TRACTS

ON THE

NATURAL HISTORY

OF

ANIMALS AND VEGETABLES.

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ANIMALS AND VEGETABLES,

TRANSLATED FROM THE ORIGINAL ITALIAN OF

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WITH

PHYSIOLOGICAL ILLUSTRATIONS, BY THE TRANSLATOR.

SECOND EDITION.

VOLUME II.

Edinburgh:

PRINTED FOR WILLIAM CREECH AND ARCHD. CONSTABLE, EDINBURGH;
AND T. N. LONGMAN AND O. REES, LONDON.

1803.

OBSERVATIONS AND EXPERIMENTS

ON THE

SEMINAL VERMICULI OF MAN AND OTHER ANIMALS, WITH AN EXAMINATION OF THE CELEBRATED THEORY OF ORGANIC MOLECULES.

CHAP. IV.

REFLECTIONS ON M. DE BUFFON'S OBSERVATIONS— COMPARISON OF SEMINAL VERMICULI AND THE PUTREDINOUS ANIMALCULA OF SEMEN.

LET us continue to examine the other principal results from De Buffon's observations, that is, the appearance of those very long and slender filaments which the vermiculi drag along in their course; the contraction and disappearance of them; the activity the vermiculi acquire with age; the facility of motion in every direction; the change of form, division, diminution of size; and, lastly, their total destruction in a few days.

To begin with the first: Leeuwenhoeck, myself, and other naturalists, have remarked that each vermicule in the semen of man, and of several other animals, has a long appendage which it drags along as it swims. But this appendage is not as Buffon would incline, a thread or long corpuſculum, without analogy to a tail or any other member, and entirely foreign to the ſpermatic vermiculus. I have always found, as has been ſhewn in the firſt chapter, that the appendage has every characteriſtic of an actual tail. It has the ſhape, and the diameter always becomes greater on approaching the body to which it is united, ſo as to form one whole, as is ſeen in tailed worms; and the vermiculi alſo uſe the tail while ſwimming through the ſpermatic fluid, bending it firſt to one ſide, then to another, and in every different direction as aquatic worms are wont to do while they ſwim. I have ſeen this innumerable times, and in the moſt diſtinct manner; ſo that I muſt diſcredit my eyes, if I think or write differently.

It is true that microſcopic obſervation on the tail of human ſeminal vermiculi is the moſt nice and delicate of any I have made: it demands the greateſt care and the ſtricteſt attention. The tail is wonderfully ſlender, and at the ſame time tranſparent; whence, too ſtrong light confounds it with the ſeminal fluid, ſo that we entirely loſe ſight of the whole. In the firſt place, the choice of light, adapted for obſervation, is of the greateſt importance. The direct light of the ſun is too powerful, as is that of a lamp, unleſs it is moderated by ſome method. That which I have found moſt fit for this fine obſervation, is the light of a window

window opposite a white wall moderately illuminated: as, for example, when exposed to a part of the sky covered with white clouds. Secondly, the finer and thinner the sliders are, on which the drop of semen is deposited, the easier are the tails discovered: I prefer talc to chrystal. Thirdly, the drop should be as thin as possible, otherwise the origin of the tail will not be discerned; the rest is concealed in the liquid; and when the seminal fluid is turbid, it is necessary to dilute it with a clearer portion. Fourthly, when the vermiculi swim, as the tail is always a little lower than the body, we must depress the focus of the lens to observe it. Finally, a microscope of a single lens, such as that called Leeuwenhoeck's, should positively be preferred to the compound microscope.

Although M. de Buffon, in his observations, mentions only one or two of the precautions alluded to, and which he seems to have used, I am unwilling to think he neglected the rest; for the exception of only one would prevent him from making the real observation. He says he always used the compound microscope in examining the semen of man and animals. I doubt not that his microscope was as excellent as he affirms; but it was a compound one, and had all the defects of compound microscopes, among which is specially placed, the object never being seen so distinct, or its outlines so well defined, as with a perfect microscope formed of a single lens. This is a fact acknowledged by all observers, and my own observations on seminal vermiculi with both microscopes confirm it more. Using the first, I saw the vermiculi precisely as has been described: with the second, I had a confused view of the body, and was frequently in doubt whether it was round or oval; the outline was always in a sort of mist or cloud. The tail, which is infinitely more delicate, appeared less sensible, and could only be distinguished as a very long slender body. It is not surprising, therefore, that M. de Buffon calls this part of the vermicule a foreign substance, a kind of long, delicate, subtile filament, since it truly appeared such to him when viewed with the compound microscope.

It may perhaps be thought that my compound microscope was not so good as M. de Buffon's; but those I use are the most perfect now made in London. I was desirous to view spermatic vermiculi with the microscope M. de Buffon had employed, that is, Cuff's microscope, which is precisely that of Mr Needham, and which, M. de Buffon observes, he used in his examination of the seminal fluid. But it showed me nothing more; and I may affirm that my observations and experiments on seminal vermiculi, as well as on the animalcula of infusions, and similar

lar beings, could never have been exempted from uncertainty, I will even say error, by preferring the compound microscope to that of Leeuwenhoeck.

Let us look back a moment to establish the certainty of tails in the human seminal vermiculi. The fact is confirmed in so convincing a manner by the experiment on talc, where the tails appear complete, and not confounded with the semen, as to put it beyond all doubt was there no other proof. However, we may observe that such extreme circumspection, and so many precautions are unnecessary to distinguish the tail in the vermiculi of many animals, when Leeuwenhoeck's microscope is used.

To the contraction and entire disappearance of the tails, which Buffon says he observed when the vermiculi remained long in the semen after it came from the animals body, I can oppose nothing except that in all my observations, which have been innumerable, the reverse was uniformly seen. The vermiculi constantly preserve this member not only while alive, but long after death; and it does not begin to spoil or be destroyed till the vermiculi themselves arrive at the same state. Further, neither the maceration of the dead bodies by boiling, nor is freezing sufficient to destroy their structure or figure. Vinegar itself and urine, fluids which instantaneously destroy the contexture of most infusion animalcula, cannot, before a considerable time, affect the tail or body of our vermiculi. We should, therefore, have to deny the existence of all these facts to agree with M. de Buffon concerning the contraction and disappearance of the tails. That my observations may have greater weight in establishing the certainty and continuance of these tails, I shall cite the authority of the great physiologist of Berne: 'Nunc quod caudas attinet, 'perpetuam particulam vermiculi spermatici, eæ 'nimis certos, et fide dignissimos habent testes, 'quibus meum suffragium per experimenta na- 'tum addidisse liceat (1).'

The other phenomena of spermatic vermiculi, observed by M. de Buffon, such as an increased activity acquired with time, their change of shape, diminution, division and duration of life from four to eight days, seemed to me no less paradoxical, particularly on attending to the repeated observations mentioned in the first chapter, and recalling those of the most acute Leeuwenhoeck. M. de Buffon's acknowledged merit at the same time prevented me from considering this assemblage of facts as chimerical; and unable to ascribe it to the fault of his microscopes alone, for, however defective they might be, they could not occasion

(1) Haller, Physiologia, T. 7.

occaſion ſuch a difference in the phenomena, I determined to reſolve my doubts by taking the trouble to make a new courſe of experiments on human ſemen and that of other animals. But notwithſtanding all my care, precautions, and all poſſible vigilance, I could diſcover nothing new, at leaſt eſſentially affecting the facts I have ſpoken of. Yet, with reflection on Buffon's obſervations, and the repetition of my own, I could not reproach him with ſeeing what did not exiſt. I thought the whole might be an equivocal effect, which ſeemed the more likely, as the phenomena which he ſays he obſerved in ſeminal vermiculi might be occaſioned by beings of a very different nature. My experiments on infuſions ſuggeſted it. I had remarked, that there is no part of an animal infuſed that does not give exiſtence to a particular kind of animalcula. They are produced indifferently by the muſcles, brain, nerves, membranes, tendons, veins, and arteries; alſo by the blood, ſerum, milk, chyle, or ſaliva, mixed with water or even by themſelves. I had not yet made experiments on human ſemen for a ſimilar purpoſe, but it was moſt probable that the putrefaction of it would give exiſtence to particular beings: and who knows, ſaid I to myſelf, that they have not inadvertently been confounded with ſeminal vermiculi, and that M. de Buffon has aſcribed to the latter the properties and phenomena exhibited by the former? Perusing Haller's Physiology again, I found he was of this opinion; nay, that he suspected M. de Buffon had never seen the spermatic vermiculi. 'Porro Buffonius, ut cum illustris viri venia dicam, omnino non videtur 'vermiculos seminales vidisse. Diuturnitas enim 'vitæ, quam suis corpusculis tribuit, ostendit non 'esse nostra animalcula (id est spermatica) quibus 'brevis, et paucarum horarum vita est.' And in another part of the same volume, he adds, 'Ea 'enim experimenta (id est Buffonii) manifesto ducunt ad animalia putredinosa (1).' However, I thought of ascertaining this fact, by observing what successively happened to semen long preserved in a watch-glass.

My first experiments were on the human seminal fluid (2). The vermiculi died in three hours and a half, and were precipitated to the bottom of the glass. On the sixth day, the fluid began to exhale a fœtid disagreeable odour: but no animated being was perceptible. The bodies of the vermiculi were seen, apparently very entire, in some drops taken from the bottom of the glass. The seventh and eighth days, there was no change; but the fœtor increased. On the ninth, I discovered very minute animalcula, their

(1) Tom. 7.

(2) 11 February, The Thermometer 45°.

their size nearly equalling that of spermatic vermiculi; but they had no tail, and much resembled most minute spherules. Like infusion animalcula, they sometimes stopped at little fragments of corrupted semen: sometimes their course was very rapid, retrograde or rising and sinking in the fluid: In a word, they possessed every property of animalcula. They were seen in every stratum of the semen, and those at the bottom moved the dead bodies of vermiculi, which were still entire, and remained so some days longer. In the seminal animalcula, we remarked the same periods with regard to increase, diminution of numbers, and termination, as is generally observed in the history of other animalcula; only, when the globular animalcula decreased, there came two other smaller generations: the last, which could scarcely be discerned, continued until the eighteenth day.

While examining the semen in this glass, I observed a portion of the same kind that had been put in another glass, and placed in a stove, that it might experience a greater degree of heat (1). The effect was such as we might expect. The vermiculi lived longer, and the infusion animalcula appeared sooner: the former died in five hours, and the latter appeared in five days. They were

(1) The thermometer at temperate.

were of a globular figure, had no tail, and all of the same size and species as those of the preceding observation. The other two colonies then appeared, the globular always remaining.

Having again prepared the same kind of semen, I had an opportunity of seeing the effect of heat in accelerating the production of animalcula (1). Some were found in the fluid in less than twenty-three hours after it was taken from the dead body. These were of a different species from the globular; they were three times as large, and of a cylindrical figure. The body in swimming undulated like a serpent, which did not occur, or was unobserved, in the globular animalcula. The dead vermiculi seemed to be their food; because the animalcula incessantly in motion surrounded them, and pecked at them with the anterior part of the body. In three days, other animalcula as minute as the spermatic vermiculi appeared, along with the cylindrical; and I remarked a circumstance which in semen I had not observed before. In the Tract on infusion animalcula, the propagation of many species by natural division has been spoken of at large. It has been said, that, in several, the division begins in such a manner that the animalcule is gradually cleft across, until it is divided into two equal parts,

(1) 22 May, The thermometer 65°.

parts, which become two animals ſmaller than the firſt: but we had to learn whether this mode of propagation could happen with animalcula ſimilar in ſize to ſeminal vermiculi. Several ſcores divided tranſverſely before my eyes; and the diviſion or propagation continuing ſeveral days, the ſeminal fluid, which had become exceſſively fœtid, now teemed with life. But the number of theſe, as well as of the cylindrical animalcula, gradually diminiſhed, in the ſame way as the numbers of infuſion animalcula decreaſe; and on the twenty firſt day, all had diſappeared. There only remained an univerſal obſcure fermentation of the ſemen in no particular direction, but the ſeminal molecules were tumultuouſly driven about to every ſide. I was not long in perceiving that this irregular agitation was occaſioned by a multitude of moſt minute animalcula concealed in the ſemen, which their courſe had put in motion; and of this there was complete evidence by diluting the ſemen with water, as they were then accurately diſcerned, appearing about half the ſize of vermiculi.

The phenomena diſcovered in the ſemen of the horſe were analogous to thoſe in the human ſemen (1). The vermiculi lived ſeven hours, and then were precipitated to the bottom, where they remained

(1) 26 July. The thermometer 88°.

remained long entire; the body and tail were complete for a month. In fourteen hours, the ſemen began to exhibit ſymptoms of putridity, and then were infuſion animalcula viſible; they increaſed; and on the fifth day, there were many ſpecies. One particularly demands our notice: not only did it multiply by longitudinal diviſion, but the ſize and figure changed every moment. Sometimes the animalcula contracted and became round; ſometimes they dilated and became elliptical, as I have remarked in ſeveral ſpecies of infuſion animalcula.

When this experiment on the ſemen of the horſe was made, one ſimilar was made on that of the rabbit. The vermiculi periſhed in four hours, and fell to the bottom; animalcula appeared in fifteen. Two ſpecies multiplied by diviſion; and one exhibited the contractions and inflations already deſcribed.

The ſame experiments were made on the ſeminal fluid of the ram, dog, and bull; on that of frogs and newts, with analogous reſults. In the beginning, and during the progreſs of putrefaction, each produced different animalcula, and all diſplayed numerous and different ſingularities. Their number increaſed; it diminiſhed, and became very ſmall. The animalcula were different in figure and ſize, and ſome ſpecies multiplied by diviſion; which proved that animal ſemen reſembled

sembled vegetable seeds, from the many kinds of beings to which it gave existence.

These facts afforded new light in illustrating how erroneously Buffon had ascribed to seminal vermiculi properties that belong to animalcula of infusions only. Let us collect the circumstances in a few words.—According to him, the vermiculi, after a certain time, were deprived of their tails. He should have said, the animalcula of infusions came in the place of the vermiculi, which were already dead and precipitated to the bottom of the liquid. He was arrested by their first appearance, and took them for seminal vermiculi deprived of the tail, which in truth they often very much resemble.—When disengaged from the tail, he adds, they acquired much greater activity. This was a necessary consequence of the first mistake. When the animalcula had come, their increased velocity could not be unobserved, since they move with much greater rapidity than vermiculi; and the erroneous supposition being admitted, Buffon had to relate, as he has done, the remainder of the phenomenon. He had to speak of the imaginary changes of the vermiculi, of their division and diminution, with the more confidence, as his opinions were confirmed by a repetition of his experiments, if not on all, at least on many species of infusion animalcula in the semen.

I think

I think I have had too numerous and too decisive proofs to doubt that the phenomena seen in infusion animalcula of semen are totally different from the phenomena exhibited by seminal vermiculi. May I not at once be permitted to oppose my experiments to those of the celebrated De Buffon? I observe that all the seminal fluids he has studied, and where he has discovered the phenomena of which we have spoken, have also been studied by me. I perceive that even more have been examined, and those both of cold and warm blooded animals. His observations on seminal fluids were only made at certain times, and in one season. I judged it necessary to extend mine to all seasons. My microscopes were not inferior to his, nay, they were much better. How then is it possible, that in the same circumstances with the French naturalist, and even in situations more advantageous for examining the phenomena of seminal vermiculi; how is it possible, I say, that these phenomena were never observed? that I never observed some, or remarked a single one? —What do I say? Not only have I never seen any of the phenomena, but beheld what completely contradicted M. De Buffon's observations, in all the fluids, many, many times examined. Such for example is the imaginary activity acquired by vermiculi, in proportion to the time they remain in semen; since directly the reverse happens, as has

has been obſerved and is mentioned in the firſt chapter. For after expoſure to the air, their motion, which before was quick, becomes much more languid; and its greateſt rapidity is when the ſemen comes from the animal's body, which, as I have already ſaid, was remarked by Leeuwenhoeck.

Buffon ſays, he obſerved the phenomena three or four days after the ſemen came from the animal, even on the eighth day in that of the rabbit. This muſt be impoſſible, becauſe the vermiculi living longeſt, as thoſe of the human ſemen, which were examined by M. de Buffon, cannot exiſt above ſeven or eight hours in the open air. When ſecured againſt the influence of air, in glaſs tubes hermetically ſealed, they do not ſurvive three days, as we ſhall afterwards ſee. Thus it is certain, that the phenomena which the author obſerved in his experiments could be exhibited by none but the animalcula originating in the ſeminal fluids when on the verge of corruption, or when they do corrupt, as ſucceeds in other liquids that will corrupt, or are already corrupted.

I cannot ſuppreſs my ſurpriſe, that the celebrated French author never doubted, whether the animated beings which he beheld in ſemen were really ſpermatic vermiculi, or only animalcula originating there, that is, infuſion animalcula. Theſe animals, he well knew, originate no

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less in animal than in vegetable infusions when beginning to corrupt; for he says, that in two infusions, one made with the testicles of a ram in water, and the other with those of a dog, he, in some days, found living animals similar to what he had seen in the semen of animals: that is, globular or ovular, without tails, moving with great activity, and often changing their shape(1). If the animalcula of the two infusions were exactly similar to those he had observed in seminal fluids, how did he not suspect, that instead of being spermatic vermiculi, they might be infusion animalcula? He had additional reason to think so, for he must have remarked that the changes of figure, divisions, diminutions of size, were not to be seen in recent semen, but in that kept some time, and about to corrupt. Of this he must have had certain indications, and an indubitable proof, from the foetid and cadaverous odour which the semen then exhales, which also, is convincing evidence that animalcula are produced in it on account of its putrescence; consequently that we cannot confound them with seminal vermiculi. Another precaution might have occurred, and saved him from error, had he chose to adopt it, namely, to examine the bottom of the seminal fluid and not the surface alone. There he would have found

(1) Histoir. Natur. tom. 3.

found the vermiculi entire though dead, which would have demonſtrated, that the animals he then ſaw in motion could not be ſeminal vermiculi.

But all I have hitherto ſaid receives greater weight from the compariſon that has been made between the vermiculi and animalcula of putrid ſemen. In another work, I have ſhewn that a conſiderable part of infuſion animalcula appear by the microſcope an aggregate of minute veſicles, of different ſizes, and more or fewer inveſted by a common pellicle, forming the exterior of the animal; that the pellicle and its veſicles are loſt and deſtroyed when the animalcula die; and if, while alive, they are wet with urine or vinegar, the body is deſtroyed and reduced to nothing (1). All theſe things are amply verified in the putredinous animalcula of ſemen; but, with the utmoſt care and attention, I have never been able to ſee any thing like it in ſeminal vermiculi. The texture of the body and tail is not vaſcular; it is uniformly homogeneous, connected, equally ſolid, and compact. For this reaſon perhaps the dead vermiculi fall to the bottom of the ſeminal fluid, and the infuſion animalcula commonly ſwim. The vermiculi likewiſe continue long entire after death; urine, vinegar, even boiling, cannot diſſolve or decompoſe their ſubſtance.

 From

(1) Saggio di Oſſervazioni Microſcopiche.

From all this, and from all that has already been said, we must conclude that the animalcula of infusions, consequently those of putrid semen, are of a nature and constitution essentially different from seminal vermiculi. That it is truly so we may easily determine, since a fluid which affords a salutary dwelling to the one is fatal to the other. Putredinous seminal animalcula originate and live in corrupted semen, but die in that which is recent and entire. On the other hand, vermiculi live with safety in recent, but perish when put in corrupted, semen. The animalcula become more lively and active when water is mixed with the semen; the vermiculi, at least several species, become motionless and die. Of the whole of these facts I have often convinced myself: Whence, I conclude infusion animalcula and spermatic vermiculi to be two different species of animals which we cannot confound without outraging nature.

CHAP.

CHAP. V.

DEDUCTIONS FROM THE FACTS STATED.—M. DE BUFFON'S OBJECTIONS ANSWERED.

As I have taken the liberty reſpectfully to remark the errors of M. de Buffon, I muſt ſtill requeſt permiſſion to ſhow the deductions that may be made from his noted theory of organic molecules. But to do this with the greater ſucceſs, it will be neceſſary to bring under our view ſome of the chief points of that theory.

The illuſtrious French naturaliſt ſuppoſes that every animal and vegetable ſubſtance includes a number of organic molecules, that is, active and incorruptible particles. He thinks they ſerve for the nutriment, increaſe, and multiplication of all beings, whether entering the body of animals by means of food, or the ſubſtance of plants with the juices they imbibe; that they intimately penetrate every part, unite there, and are indentified, if we may ſay ſo, and afford nouriſhment to the plant or animal. If either is young, it appropriates all the organic molecules, incorporating them with itſelf; and, by extending and expanding the fibre, they effect increment. If the being is already an adult, if it no longer is ſuſceptible of expanſion, then all the molecules being unemployed in nutrition, thoſe which are ſuperfluous are depoſited in the organs of generation, and ſome appropriated for propagation of the ſpecies; an event which ſucceeds when the organic molecules of the male are mixed with thoſe of the female in the matrix, ſo that the moſt analogous tend to approach each other, and, uniting in virtue of certain relations, form individual wholes, reſembling, as in miniature, the different parts of the two individuals in which they are modelled; from all theſe individual wholes together, there reſults a general whole, which is the embryo.

If the organic molecules afforded by the male are more numerous than thoſe afforded by the female, or more active, the embryo will be a male; if the molecules of the female are more numerous, it will be a female. The greater number derived from either will occaſion the greater reſemblance in the embryo to the individual from which it has received them.

Large animals are leſs fruitful than ſmall; the reaſon is evident. The former extract fewer organic molecules than the latter. A bull draws leſs nutriment from hay and ſtraw, and conſequently

quently fewer organic molecules than a bee does, in proportion, from the finer parts of flowers.

Animals covered with ſcales are more productive than thoſe covered with hair, probably becauſe, by perſpiring leſs, they accumulate a greater number of organic molecules.

If, inſtead of collecting in the organs of generation, the molecules are carried to other parts of the animal, they there form minute living animals, as *teniæ* and *aſcarides*, worms ſometimes inhabiting the inteſtines, the liver, and the ſinoſities of the brain.

Thus does the theory of M. de Buffon explain theſe phenomena, and ſome others, which, for the ſake of brevity, I ſhall omit. Wiſhing this theory, the offſpring of his fertile genius, to be adopted by nature, he recurs to the ſeminal fluids of animals and the infuſions of plants, becauſe in both, according to his opinion, are organic molecules clearly exhibited under the form of globular, ovular, or other ſhaped corpuſcula, endowed with motion, ſubject to various changes of figure, dividing into ſeveral ſmall bodies, and then acquiring greater activity, which augments more and more, as they are further decompoſed, until their minuteneſs renders them inviſible.

This laſt trait of M. de Buffon's theory proves that it reſts entirely on the facts related by its author, that is, on a falfe hypothefis. For, with refpect to infufions, we have feen that there is nothing in them to indicate organic molecules, as the moving corpufcula there are actually animals, fome viviparous, others oviparous, and as thofe, multiplying by divifion, do not produce that progreffion of fucceffive diminution which Buffon imagined he faw, but the fmalleft grow larger like other animals.

Having found the living putredinous beings of femen to be exactly of the fame kind as thofe of infufions, by a direct and conclufive confequence it follows, that they could never be confounded with organic molecules; and we may fay the fame of feminal vermiculi, whofe animality has been fufficiently proved by the facts related in this treatife, and thofe engroffed in the fubfequent chapter.

M. de Buffon's theory is thus completely deftroyed. Such is too often the fate of the hypothefes of ardent and fanciful philofophers, firft invented and then fought for in nature. This ingenious naturalift, diffatisfied with the theories of generation already framed, and hurried on by his tafte for novelty, imagines an animated matter in bodies, original, incorruptible, and always active, which he fpecioufly denominates *organic molecules*. Making them act according to certain terms, and with a certain effect, he thinks

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he can explain the great work of generation, and the most inscrutable phenomena it presents, and employs all that powerful and persuasive eloquence which characterises him as the orator of the age. Prejudiced in favour of his theory, it was not difficult for him to find it in nature. His views were less directed to see what actually existed than to what he wished to find; not otherwise than his celebrated countryman, the reformer of botany, who fancied that metals and stones vegetated, and thought he had evidence of this imaginary vegetation, that he saw seeds and plants where there were none. If this learned academician, who has ever possessed my full esteem, will take the trouble to repeat his experiments on the semen of man and animals, with better microscopes, and, forgetting his favourite organic molecules, impose a rule on himself to receive as truth nothing but the images transmitted by the senses, without adding the corrections of imagination, as is the duty of a veracious naturalist; I can assure him he will find all that I have witnessed a thousand times, and described so largely in my works. I earnestly entreat him not to reject this without a trial, which must certainly result to the advantage of truth.

I now mean to examine Buffon's objections intended to prove that the living beings seen in spermatic fluids are not real animals. Some of them have already been anſwered in the beginning of chapter 3; and it appears, from what I have hitherto ſaid, that ſome are falſe; ſuch as, the formation of the animals under the obſerver's eye; the loſs of their tails, and diminution of ſize. Two yet remain to be ſpoken of; their diviſion into parts, and their frequent change of ſhape. Theſe phenomena are real, though never obſerved in ſeminal vermiculi, and only in the animalcula of infuſions. M. de Buffon eſteems them incompatible with the character of animality.

With regard to the objection concerning diviſion, it is needleſs to ſtop here for a diſcuſſion; and I paſs to the ſecond relative to the metamorphoſes of putredinous animalcula. It is true that the body of the animals familiar to us is ſo conſtructed that the ſhape never varies, and cannot be materially altered without deſtruction; but it is no leſs true, that there are others experiencing the contrary; as for example, among inſects, many ſpecies of worms. To be convinced of this, it is only neceſſary to glance at the operations of Nature, or to open the works of naturaliſts. How many worms of this kind have been elegantly deſcribed by Redi and Valliſneri! A ſingle ſpecies may ſerve for many, and this is the *cucurbit* worm. Is it not certain by the obſervations of theſe two, eſpecially of the Tuſcan philoſopher, that they aſſume numerous and various

rious shapes, and, to use his own words, 'Some-'times they contract and swell into purses, some-'times they extend and curve into a semicircle.' Does not Reaumur say the same of certain worms changing into flies, whose head, that part constantly the same in most animals, in this insect so wonderfully changes its appearance as to be sometimes extended, depressed, or contracted; sometimes obtuse in one part, and acute in another. Do not we daily see the same changes in earth-worms, snails, and particularly in leeches, extending the body till it is long and slender, then contracting till it becomes short and corpulent, or growing thick at one end and small at the other? What shall we say of the wheel animal, that aquatic creature, which, from its wonderful metamorphoses, we may term the Proteus of the insect tribe? If wheel animals, earth-worms, leeches, shell and naked snails, and so many species of worms (1), are not degraded from the rank of other animals, notwithstanding their different mutations of figure, the same should be the case with the living beings in putrid semen (2).

Two

(1) The author does not mean that these animals belong to the class of vermes.—T.

(2) Notwithstanding this, Leeuwenhoeck observes, 'Al-'though seminal animalcula are shaped like an eel, they can 'contract their bodies so much as to become round.'—T.

Two other objections are still to be appretiated; one deduced from the singular motion of seminal vermiculi; the other from the different effects of heat and cold upon them. Let us attend to the first. An animal, according to M. de Buffon's reasoning, is subject to change its inclinations. Sometimes its motion is quick, sometimes slow, then it stops and rests. But nothing of this occurs in seminal vermiculi: they are in continual motion, never rest, and, when once they stop, it is for ever. Hence they cannot be animals. This objection is, like the former, both being founded on the analogy of the large animals best known to us; which, as they do not change their shape, naturally have the vicissitudes of rest or motion, of accelerated motion, or the like. But to admit that such vicissitudes are certainly a characteristic quality of animals, it is improper to consider these alone; it is necessary to examine other genera and other species, and to dwell particularly on the smallest, especially those inhabiting fluids which have more analogy to spermatic vermiculi. It is undoubted, that among them M. de Buffon would have found several animals, which, far from having the alternatives of motion and rest, are naturally in motion; so that their life seems to consist in a perpetual motion. To ascertain the fact, it is necessary, during summer, to observe the water of marshes,

marſhes, ponds, and ditches, where all inſects are. There we ſhall ſee ſome in motion by conſtant contorſions; as, for example, the worms mentioned by M. Trembley, ſerving polypi for food: the body is continually in oſcillation (1). Without the trouble of ſeeking them in the country, M. de Buffon may at his leiſure obſerve it at home in the eels or ſerpentuli generally found among vinegar. If a ſmall portion of this liquid is put in a thin glaſs veſſel, and expoſed to the light when the ſun ſhines bright, on examining the higher parts with a magnifier, where the eels are more diſtinctly ſeen, their contorſions and undulations appear inceſſant; they dart from one ſide to another, and continue thus from morning to night; in this manner do they perſevere ſeveral months, that is, to the end of their lives. It would ſeem, from theſe facts, that the perpetual motion of ſeminal vermiculi cannot be a ſufficient reaſon for proving they are not animals. But, farther, ſuch motion is not natural to the vermiculi; it is forced and violent; for they have quitted their native abode to enter our atmoſphere; they experience the lively impreſſion of the air, which hurts and injures them, and conſtrains them to perpetual flight. Doubtleſs the air is noxious, and occaſions their continual motion,

(1) Memoires ſur les polypes.

tion, as is proved by facts. To fix the point, I waited till the human femen cooled. I then took some drops, and spread them very thin on a watch-glass. The vermiculi of the spread drops, though these were of considerable thickness, constantly perished sooner than those of the whole semen; because, as I think, the former were more exposed to the air. Two equal portions were taken at the same time, and the one put in a close and the other in an open vessel: the vermiculi of the latter always died sooner. The privation they underwent clearly ascertains how injurious this element was to them. They lived longer in vacuo than in the open air; so that all were dead, when some still lived in a vacuum. The difference of time in their death was an hour and a half, two, even three hours, and sometimes longer, according to the season of the experiments. These facts prove, that the air is noxious to the vermiculi; and the following evince that it is the cause of their being in continual agitation. Capillary tubes were made at the blow-pipe, and one end immersed in recent semen; it ascended the cavity of the tube to a considerable height. Breaking the tubes near the part to which it rose, I presented this extremity to the blow-pipe, and instantly sealed it. The same was done to the other end, by which means the seminal fluid was deprived of all communication with the external air.

air. The tubes were so drawn out, that the thinness of the glass permitted me to see the vermiculi within. The peculiarities they presented were very different from those of the rest. All, or at least most of them, had a singular mode of moving; some had that kind of motion observed in those exposed to the open air; others had a continual irregular motion, changing from velocity to inaction and reciprocally; others stopped entirely, and, after resting several minutes, resumed their former rapidity: we did not see them inconsiderately strike against the solid portions of the semen, as was remarked in the first chapter, but always avoiding them and turning aside or retreating. These peculiarities indeed succeeded better, and with more uniformity, when the tubes were kept warm. I have before said, the longest period of life of the human vermiculi is seven or eight hours when exposed to the open air; but how much is it prolonged when they are included in tubes? In summer I have succeeded in preserving them two days or more, and in spring and autumn almost three (1).

It

(1) Authors dispute very much concerning the duration of the life of these animals. Some have supposed they live only a few hours, and others, several days. Leeuwenhoeck is a most exact observer; he says, he put the semen

It will undoubtedly seem paradoxical, that the vermiculi live longer in spring and autumn than in summer. The reverse apparently should happen; for the heat of summer should be more congenial to them, as it approaches nearer to the natural heat of living man. At first, it gave me considerable surprise, nor was it diminished on reflecting, that in open air they live much longer when the weather is warm. This induced me to repeat my experiments. And I constantly found, that, during summer, they never lived as long in tubes as during spring and autumn. In summer, they absolutely die sooner on the warmest days. With a little reflection, it is not difficult to comprehend the cause of the difference. We have seen that the semen of man and animals, when removed from its natural situation, very soon becomes putrid, and putrefaction takes place earlier as the heat is greater to which it is exposed. To this cause, therefore, I ascribe the more immediate

men of a dog in a glass tube during summer; many *animalcula* were dead the first day, more on the second and third, and on the fourth very few were alive. Next year, he examined some semen taken from the same dog; for seven days and nights, some *animalcula* were still alive, and a few swam with as much velocity as if they had recently come from the animal. *De diuturna vita animalculorum in semine masculo canis.*—T.

immediate death of vermiculi in capillary tubes during ſummer. Having filled ſimilar tubes with recent ſemen, and ſealed them hermetically, I expoſed ſome to the heat of the atmoſphere at about 63°, and others to the heat of the human body, keeping them in the axilla in a large glaſs tube. In a ſtate of health, my own heat is about 97°. The vermiculi expoſed to the heat of the atmoſphere lived two days and a half; ſome even three; but thoſe experiencing the heat of my body ſupported it only thirteen hours. Their more immediate death cannot be aſcribed to the greater degree of heat, ſince it is about the ſame as what preſerves them alive, or in which they generally live: Nor can we aſcribe it to any other noxious principle ariſing from the nature of the tubes, ſince they were perfectly ſimilar to thoſe expoſed to the atmoſpheric temperature. It therefore becomes neceſſary to recur to ſome alteration or noxious quality contracted by the ſemen, which makes heat accelerate their death: and I acknowledge, that I cannot find this alteration or injurious quality but in the principle of putrefaction, which is manifeſted by the fœtor on breaking the tubes: certainly it is this that muſt be fatal to the vermiculi, as I ſhall demonſtrate in Chapter 6. The putrefying principle does not commence in the ſemen expoſed to the open air only ſeven or eight hours in ſummer.

On

On the other hand, as the heat of summer nearer than that of any other season approaches what the vermiculi experience in us, we clearly see why they then live longer in the open air than at any other time: and for the same reason we may understand why life is abridged in proportion as the cold increases.

But it is time to come to the objection of heat and cold which the author thus proposes. On exposing the semen to the cold air, the vermiculi did not seem to suffer from it. They continued moving with their usual quickness as long as those not exposed, though the fluid had acquired that degree of cold in water on the point of freezing, as one might be convinced by touching it. On the contrary, if the same vermiculi suffer heat, their motion ceases, although the heat is moderate. In consequence of these facts, if the vermiculi are real animals (I am relating Buffon's reasoning) they will exhibit an appearance and constitution very different from the appearance and constitution of other animals; as too much cold relaxes and destroys motion, whereas mild and moderate heat preserves it.

We must regret that our author, instead of using taction to judge of heat or cold, did not employ a thermometer; for all philosophers know the touch is a very equivocal proof. He ought to have discovered precisely at what degree of of heat the motion of vermiculi ceafed, and the degree of cold by which it is not relaxed. I have efteemed it moft effential to fupply this defect, in order to obtain arguments more conclufive and decifive, and to learn if, with refpect to heat and cold, the vermiculi are of a nature and conftitution much different, as M. de Buffon imagines, from the nature of other animals.

Though the obfervations in Chapter I. do not feem to beftow that vigour of conftitution fufficient to enable them to refift cold, fince it feemed their motion decreafed with the decreafing heat of the atmofphere, fo that when the thermometer ftood at 36°, they continued moving only an hour and a half: yet I refumed my experiments, refolving to extend them further, and fubject the femen to a degree of cold equal to freezing, and carefully obferve what fhould happen to the vermiculi. The femen firft ufed was that of the horfe. On the 14 of January, it was expofed to freezing: the vermiculi could not be more vivacious; their vivacity evidently diminifhed; and in fixteen minutes all were motionlefs, though the femen was not frozen. The cold becoming more intenfe on the 18, I repeated the experiment. The vermiculi became motionlefs in eleven minutes. The thermometer ftood at 8° below freezing, but the femen was ftill fluid. The experiments were often repeated during winter. They constantly proved, that the duration of motion was proportioned to the temperature of the atmosphere.

In prosecuting my experiments on the semen of the horse in summer, a thought occurred of subjecting vermiculi to the cold of freezing, by immersing the glass that contained them in snow. The same effect was produced as by the winter's cold, that is, in fourteen minutes, it made them motionless; though, when exposed in another glass to the heat of the atmosphere, they moved seven hours and a half. An accident that happened in the summer experiments afforded new intelligence, and divested me of a prejudice. Observing that the vermiculi had become motionless, I took the glass from the snow, and left it exposed to the air at 81°. An hour after, it astonished me to find all the vermiculi reanimated in such a manner as if they had just come from the seminal vessels. I then perceived the cold had not been fatal, but had reduced them to a state of complete inaction. They were replaced among the snow, and in three quarters of an hour being taken out, I observed these phenomena: In a few minutes, their vivacity relaxed, and the diminution increased until they lost the motion of progression, and retained that of oscillation only, which also ended in a few minutes more. Exactly the reverse was seen, when they passed from

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the cold of snow to the heat of the atmosphere. The first motion that appeared was oscillation; the body and tail began a languid vibration from right to left, and reciprocally; motion was then communicated to the whole vermiculus. At first, it was scarcely perceptible; it soon increased, and grew very considerable. It should likewise be added, as cold does not render all the vermiculi motionless at the same moment, but some later than others, neither does heat affect all with equal power.

I subjected the seminal fluids of man and the bull to the same experiment, and had the same results as from the semen of the horse, except that a degree of cold less than freezing immediately destroyed all motion in the vermiculi of the bull.

On approach of the following winter, the same experiments were resumed; and I succeeded in reanimating torpid vermiculi by breathing on the semen, by applying my finger to the tube on which some drops were put, or by placing it near the fire. Removed from this reviving heat, they fell into the same lethargy as when in summer they were transmitted from the atmospherical temperature to the cold of snow. During the rigorous season, I exposed them to a more severe trial: they were exposed to cold above 9° below freezing. As might have been expected,

 it

it immediately rendered them motionleſs. In five minutes, not a ſingle vermicule moved. When they had been expoſed five minutes longer, they were tranſported into warm air, and left there for ſome time. Although this intenſe cold continued ten minutes, the ſemen was not frozen, but it had fatally injured a complete third of its inhabitants. They evinced no ſign of life when removed to, and kept long in, a warm ſituation; on the contrary, they had all the appearance of death. The other vermiculi recovered indeed, but their motion was languid in compariſon to what it was before. The experiment was made 15 December, and repeated on the evening of January 5 at 10° under freezing.—In about a quarter of an hour, I perceived the ſemen begin to congeal about the edges of the glaſs; I then put it in a ſtove, but it was an ineffectual remedy for the vermiculi. Not one recovered; and thoſe environed by the ice periſhed as well as thoſe in the fluid parts. The like happened to the vermiculi of other two glaſſes, on which I made the ſame experiments this evening, notwithſtanding care was taken to regulate the different degrees of heat, leſt too ſudden tranſition from heat to cold might be injurious.

Such were the experiments with cold: any one may draw the concluſions. Very far from excluding vermiculi from the rank of animals, it ſurpriſingly

ſurpriſingly confirms them in it. For what can more ſatisfactorily prove it than ſeeing languor become more immediate as the cold is more intenſe; ſeeing the vermiculi revive when brought to heat; and to witneſs their actual death, when the cold is of a greater degree? Such is the ſtate of moſt ſmall animals, deprived of action and rendered torpid by cold; with heat, they recover life and motion, and yield under cold ſtill more intenſe.

How can theſe facts, multiplied, repeated, uniform, conſequently certain and incontrovertible, ſubſiſt with the aſſertions of Buffon, who ſuppoſes cold does not impede the motion of ſeminal vermiculi? Inſtead of negativing this illuſtrious Frenchman's affirmation, I think there is a method of conciliating our obſervations. We have already remarked the error which occaſioned his confounding vermiculi with animalcula, and aſcribing to the former the properties pertaining to the latter only. It is very likely, that the effect of cold he has obſerved on vermiculi is alſo a conſequence of the ſame miſtake; and this is the more probable as it is ſeen in putredinous ſeminal animalcula. Nor do the animalcula of infuſions alone, at leaſt many ſpecies, withſtand cold of a great degree; for thoſe found in putrid ſemen are undoubtedly of that number. Several experiments convince me of it; but to avoid the ennui of my reader, I ſhall not detail them. There is one circumſtance which ought not to be paſſed in ſilence; that although theſe animalcula can ſupport more cold than ſeminal vermiculi, their motion does become languid; and, on increaſing it conſiderably, they periſh like inſects which yield to the greateſt degree of cold.

When I found theſe methods of conciliating Buffon's experiments and my own, with regard to the phenomena from cold, I attempted to find the ſame reſpecting thoſe exhibited by heat, but that was impoſſible. My obſervations have been directly oppoſite to his. Thoſe he made are comprehended in a few words: "The motion of "vermiculi ceaſes when they are expoſed to a "ſmall degree of heat." I entreat the reader to examine mine, that he may be enabled to compare and form an opinion.

Two watch-glaſſes were put on water contained in a veſſel, one containing a portion of recent ſemen, full of vermiculi; the other, an equal quantity of ſemen, old and ſwarming with putredinous animalcula. To know the ſucceſſive degrees of heat, I had put the ball of a thermometer into each glaſs. The water was gradually heated on a ſlow fire. As the thermometers roſe, I took ſome drops of ſemen from the glaſſes for examination with the microſcope. The putredinous animalcula were very vivacious at 99°; at 104°, their

their motion began to grow languid; at 106° and 108°, all perished. Seminal vermiculi are of a more hardy constitution: they were active at 111°; some began to perish at 120°; and at 131°, there was not one alive: so that the difference which occasions the destruction of the one and the other is about 22 degrees. The vermiculi were those of human semen. The same experiments were made on the semen of the horse, the bull, and the dog. There was some little difference: those of the horse and dog perished at 126°, those of the bull at 133°.

A number of capillary tubes were next filled with semen, partly full of seminal vermiculi, and partly of putredinous seminal animalcula. I sealed them hermetically, and put them at the bottom of a vessel of water, which was gradually warmed. A thermometer was also immersed. When the water had attained 99° of heat, I began to examine the tubes one after another, as the heat increased. The seminal vermiculi of man, and the other animals, died at 122° and 124°; and the putredinous animalcula at 106° and 108°.

These facts demonstrate, that animalcula, originating in putrid semen, are of a constitution better calculated to resist heat, which many other animals support, and which die only at the same degree that animalcula from putrid substances die, or nearly about it. And ſeminal vermiculi, inſtead of ceaſing to move and periſhing at a ſmall degree of heat, according to M. De Buffon, ſupport as much as is deſtructive to many other animals; which far from being wonderful is rather congenial to their nature, ſince they live in warm blooded animals, that is, in an atmoſphere in general much warmer than the air, and other fluids where animals are found, and it is fit that they ſhould be able to reſiſt much greater heat.

CHAP. VI.

NEW EXPERIMENTS AND OBSERVATIONS TENDING TO DETERMINE THE NATURE OF SEMINAL VERMICULI.

I FLATTER myſelf that the reader will not be diſpleaſed, if he does not find the ſame order and connection in the reſt of the obſervations, which I have hitherto endeavoured to preſerve. It is neceſſary to conſider what follows as an appendix, which, in my opinion, muſt indiſpenſably be added to prove the animation of the vermiculi

culi with greater certainty; this being one of the chief objects propofed: and, when once eftablifhed, not only will the contradictory opinions concerning the nature of fpermatic vermiculi, already explained and difcuffed, be confuted, but we may alfo anticipate every new hypothefis that can be fuggefted.

One day, during winter, I had abundance of femen, taken from the veffels of a dead body (1); and, wifhing to preferve the vermiculi fome hours alive, put the glafs containing them in the funfhine, on the outfide of a window. The heat of the fun was 70°, which kept them alive a confiderable time: but obferving the vermiculi an hour afterwards, it gave me extreme furprife to find almoft the whole motionlefs. I knew not whether this indicated real or apparent death; and, thinking to fatisfy myfelf by expofing them to greater heat, I tranfported them near the fire. Experience had taught me how inftrumental heat is in reftoring vermiculi to motion. But it was in vain; and although kept here a long time, they manifefted no fign of life. It was otherwife with thofe left in the fhade, and thefe carried to the fire; for there was another portion of femen in a watch-glafs in the fame apartment. The vermiculi alfo had

(1) When the kind of femen is not fpecified, that of man is always underftood.

had become motionlefs, but they foon refumed their original vivacity. The fingularity of the phenomenon made me fuppofe it accidental, and I did not think of repeating the experiment during this winter and the following fpring. But I had afterwards occafion to obferve, that the fun was conftantly fatal to vermiculi in a few hours; though the intenfity of the heat did not equal the degree which, in the preceding chapter, is faid to have been fatal to them. This was afcertained by means of the fun in autumn; but the phenomenon, which I at firft thought accidental, has appeared conftant and invariable. The folar influence, at the fame time, has no quality noxious to the putredinous animalcula of the fame femen, provided its intenfity does not raife the thermometer to 106° or 108°, which alfo contributes to prove the difference between the vermiculi and animalcula.

The novelty of fuch refults was a fufficient incentive for inveftigating the caufe. Experiment having fhewn me that a certain degree of folar heat quickly kills vermiculi, though an equal degree in an apartment does no injury, I could not be perfuaded that the fimple heat of the fun caufed their deftruction, but imagined fomething entirely different. My firft idea was, the agitation of the air: I conceived that, by putting the femen on the outfide of the window, the vermiculi were

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more affected by the sensible influence of this element, and sooner yielded under it than those within the apartment, where the air was less agitated. My supposition was erroneous; for, putting out two glasses with the same semen, equally exposed to the air, and only with the difference, that one was in the sunshine and the other in the shade, those in the sun always died sooner than those in the shade. Further: a division was put in the seminal fluid of the same glass, separating it into two parts, so that one was exposed to the sunshine and the other was not; it uniformly happened, that the vermiculi of this latter portion long survived those of the other.

Attentively contemplating that in the solar rays with the naked eye, I suspected another cause. The semen both greatly diminished, and became denser; likewise, the colour changed. This increased density might be prejudicial. An easy method was employed to ascertain the fact, which was to prevent the evaporation of the semen in the sun, because the density might be occasioned by evaporation of the more volatile parts; and my purpose was attained by hermetically sealing several capillary tubes full of semen, and then exposing them to the sun, along with another portion of semen in a watch-glass. The thermometer stood at 73° in the sun. Those in the watch-

glass

glass did not live an hour; but the vermiculi in the capillary tubes retained all their vigour at sun-set, though the experiment was made in the morning; and an hour after mid-day, the heat of the sun was 104°. On the succeeding days, other capillary tubes were put in the sunshine as above; the vermiculi, as usual, were long alive. Therefore these repeated facts prove two things; *first*, That the sudden death of vermiculi in sunshine is not properly the effect of the sun, as it would have killed those in the capillary tubes as soon as those in the open vessels; *secondly*, We can ascribe their immediate death only to some vicious quality or alteration acquired by the semen, when exposed to the air, and against which it is secured in a close vessel. But as it does not seem to arise from any other cause than the inspissating of the semen, for putrefaction cannot begin in so short a time, we are induced to suppose this the sole cause of their death, at least an essential reason.

These facts are illustrated by the following experiments: Two glass tubes filled with semen to a given height, and stopped with a stopper well fitted, were placed in the sunshine: one stopper touched the semen; the other was an inch above it: the tubes were in an erect position, and an equal quantity of semen in each. In an hour and a half, the influence of the sun had occasioned no evaporation

evaporation in the tube with the ſtopper touching the ſemen; indeed it was impoſſible, as no vacuity was between them; but the ſemen in the other tube had evaporated. The inſide of the glaſs and the end of the ſtopper were covered with a thin pellicle, formed of a very tranſparent fluid, which could be nothing but the more ſubtile parts of the ſemen volatilized by heat. The quantity was diminiſhed, which could not be otherwiſe, and it was a little thicker; neither of theſe circumſtances was remarked in the other tube. Both the fluids were examined with a magnifier: the vermiculi were alive where there was no evaporation; in the other tube all were dead. Thus it is evident, the ſolar heat does not kill the vermiculi; but that their death is occaſioned by ſome noxious quality imparted by it to the ſemen, either conſiſting in that thickneſs, or ſomething elſe derived or generated on the occaſion, which alſo correſponds with the nature of animals that are injured and periſh, if the ambient fluid in which they live, aerial or aqueous, begins to alter and ſpoil.

It ſhould likewiſe be explained how two degrees of heat, equally intenſe, can produce ſuch oppoſite effects: for, the immediate action of ſunſhine kills the vermiculi, while that of a heated apartment does them no injury: but my obſervations have tobacco, the effluvia of the moſt ardent liquors, and the electric ſpark, as I have proved, we ſhall have an aſſemblage of proofs ſo convincing, ſo deciſive of the real and abſolute animality of ſeminal vermiculi, that I know not what other evidence could be required to prove that atoms, minute as points in matter, are of ſuch a nature as vermiculi ſeem to be.

I muſt juſtify myſelf for the inconſiſtency of what is now related, and the brief account of vermiculi in my firſt treatiſe on infuſions (1). There I ſpoke negligently; I had not then ſtudied them; but truſted to what others had written, and adopted the theory that ſeemed beſt ſupported by facts. I did not heſitate to embrace M. de Buffon's opinion; and, with him, ſuppoſed vermiculi were not real animals, as his ſentiments appeared to be ſupported by obſervations more numerous, better detailed, more connected and convincing, than thoſe of Leeuwenhoeck. At that time this was my opinion; and it would have always continued the ſame, had not the obſervations narrated here convinced me of the contrary; and I flatter myſelf that nobody will reproach me if my preſent ſentiments are different from what they formerly were.

This

(1) Saggio di Oſſervazioni Microſcopiche.

This chapter ſhall be terminated with ſome reflections equally curious and nice, reſpecting our vermiculi. They were communicated to me in a letter from M. Bonnet; and the reader cannot judge better of their import than by having them before him. After informing me of Linnæus' ſingular opinion, that vermiculi are inert corpuſcula floating in the ſemen, he adds, 'I return to the 'ſeminal vermiculi, whoſe exiſtence cannot be 'doubted. Of all animalcula in fluids they are 'thoſe that have excited my curioſity moſt. The 'element in which they live, the place of their 'abode, their figure, motion, ſecret properties, 'all, in a word, ſhould intereſt us in ſo ſingular 'a kind of animated beings. How are they form'ed there, how propagated, developed, or fed; 'and what is their motion? What becomes of 'them when the liquid they inhabit is returned 'by the veſſels, and mixed with the blood? Why 'do they appear only at the age of puberty, or 'where did they exiſt before this period? Do 'they ſerve no purpoſe but to people that fluid, 'where they are ſo largely diſperſed? How far 'are we from being able to anſwer moſt of theſe 'queſtions? and how probable is it, that future 'ages will be almoſt as ignorant of the whole as 'our own. If, as I have ſaid, in the *Palingeneſie*, "our world has been chiefly made for intelli-

" gence superior to our's, it is that which knows
" the history of spermatic vermiculi, and that of
" the most mysterious productions of the globe,"
' part 12 and 13. You may see in Articles 131,
' 132, 133, 134, 135, of my *Corps Organisés*,
' that in my youth I attempted to study animal-
' cula. Observe what is said on this occasion in
' Article 135, concerning those of infusions.
" Respecting the appearance of animalcula in
" substances that have been boiled or exposed
" to a degree of heat at which we cannot con-
" ceive any animal may survive, the difficulty
" ought not to surprise us too much, as it is
" founded only on our ignorance of the heat
" that certain animals may support. Besides,
" it is not certain that the animalcula were not
" in the infused substances. Perhaps they might
" inhabit the air confined in the vessel, and pass
" from it to the infusion. Perhaps there is a
" perpetual circulation of aerial animalcula in
" organized bodies, and in bodies organized in
" the air.

"No animalcula are better adapted than seminal
" vermiculi to demonstrate how well it pleases
" the Supreme Wisdom to multiply sentient be-
" ings, and leave no portion of nature void.
" Could we have suspected that this precious li-
" quid, the reproductive principle of large ani-
" mals, is at the same time destined for the
" aliment and pleasure of an innumerable mul-
" titude

"titude of most minute animated beings? Thus "has the Adorable Wisdom presided over the "formation of the universe, and known to make "the same production serve for such different "purposes.

"The Author of Nature,' 'Have I said, *Contemplation, Part. 5. Chap. 17,*' "has left nothing "useless. The pollen consumed in the generation of plants is very little compared with the "whole quantity that each flower affords. Wisdom has therefore created the industrious bee, "which uses the superfluous part of this dust "with an art that the most skilful geometers "cannot enough admire. The pollen of the stamina apparently supplies the necessities of many "other insects; and these insects are in some respect to pollen what vermiculi are to the "seminal fluid.

'The origin of certain worms in the human 'body, and that of animals, is a problem yet unsolved by naturalists: such, in particular, is the 'origin of the *Tenia*: in my Dissertation, I have 'spoken at length of this singular worm. The 'origin of spermatic vermiculi is a problem still 'more profound. However, I should much incline to presume, that, like those mentioned in 'my Dissertation, they originate from without. 'The change of temperature, abode, and nutriment, may produce first in individuals, then in 'ſpecies, very material alterations diſguiſing the 'primitive appearance to our view. A worm 'deſtined to live in the waters, and tranſported 'to our inteſtines, might not periſh, and yet be 'very much diſguiſed, eſpecially if introduced 'when young, or under the form of an egg or of 'ſemen; and if the worm was to propagate, the 'ſubſequent generations would be ſtill more diſ- 'guiſed. Let us, therefore, ſuppoſe that the ſe- 'mina of certain infuſion animalcula may be in- 'troduced into the ſeminal reſervoirs by the cir- 'culating ducts: they might be developed and 'live there. No doubt, this new abode, a tem- 'perature and aliment ſo different, may greatly 'affect the original form of the animalcula, and 'at length induce changes that may more and 'more remove them from their firſt appearance. 'All mankind had the ſame origin. What va- 'rieties, what ſtriking varieties are there in the 'human ſpecies! Let us compare the inhabi- 'tants of the frigid zone with thoſe of the tem- 'perate regions, and thoſe of this with the inha- 'bitants of the torrid zone; and we ſhould ſup- 'poſe we ſaw different ſpecies of men (1). The 'ſemina

(1) M. Bonnet extends analogy too far: and himſelf proves the dangers of analogical reaſoning. Whether there are actually different races of men on the earth, which cannot have had the ſame origin, or whether all may

'semina of some infusion animalcula are proba-
'bly so minute, that they may easily arrive at the
D 3 'reservoirs

may have sprung from one parent, may well be the subject of dispute. It belongs to the philosopher to admit nothing but evidence. Prejudice, tradition, and analogy must be rejected, else he will never attain the way to truth; and facts alone must constitute the foundation of his knowledge.

This is an important subject: it merits mature investigation. From the inquiries of modern naturalists, there is undoubtedly great reason to believe that there are conspicuous varieties among the races of men now inhabiting the globe; varieties so striking as neither to be affected by climate nor the mode of life. Independent of the size of the person, the colour of the skin and the nature of the hair, on all which climate may in general have a considerable effect, there is a difference in the figure of certain bones, and in the length of others that seems peculiar to the men. The quantity and proportion of flesh are variously distributed over some parts of the body: and one of the strongest evidences of a distinct race is the singular conformation of the Egyptian and Abyssinian women, and those of the interior of Southern Africa.

It is very possible that casual mutilations or natural imperfections may be transmitted to posterity: we have actually witnessed it on a small scale: yet I rather conceive that they would disappear in subsequent generations. But how can we suppose the numerous and important varie-

ties

'reservoirs of the seminal fluid. Apparently 'they are excluded, but in those seminal fluids 'that have acquired a certain degree of perfec-'tion, which happens only at the age of pu-'berty, it would be a most curious experiment 'to try whether infusion animalcula would live 'in some seminal fluids: And in the same man-'ner, whether the vermiculi would live in infu-'sions. Above all, the temperature of the place 'and the fluid would need to be regulated. Who 'can say that this experiment, which is certainly 'new, will not succeed? I communicate all my 'ideas to you. My maxim is to despair of no-'thing in natural history. Why should we say a 'thing is impossible, because we have not seen it 'succeed? This maxim is founded on our pro-'found ignorance of the secrets of nature, and on 'the deviations she in many cases seems to make 'from her ordinary course. Every where is an 'universal latitude seen, yet I am ignorant of 'the limits. They can be discovered by experi-'ment alone: and how much may experiments

'of

ties which we behold arise from accidents that individuals were subjected to in the most remote antiquity. Why should it be more difficult to admit variety in the races of men than of any other animated being inhabiting the globe?—T.

"of all kinds be combined, multiplied, repeated, "and perfected!"

The difficulty of the questions proposed in this valuable extract is too well defined by its illustrious author not to be evident to all who possess the smallest portion of philosophy. It will always afford me a good excuse for only attempting to answer the doubts by distant conjectures. The questions may be reduced to three. 1. What is the origin of seminal vermiculi? 2. How do they propagate? 3. What purpose do they serve?

As to the first, though Bonnet makes no positive assertion, we perceive his inclination to think seminal vermiculi have an external origin. Such has been the opinion of many authors in esteem; and such is the opinion of those who suppose that the worms in the body of man and animals originate from without. Sir Charles Linnaeus believes the abode of the *tenia* is in the waters; there he has found them very small, and even in some fishes, particularly in tench, which seems to favour this opinion (1). But we should be certain of the identity of the species found in water with that found in the human body, and of this we have not yet had sufficient proof. We

 cannot

(1) See the Italian translation of La Contemplation, part 10. ch. 26. Note, at the words, *Molte centenaia di piedi.*

cannot deny, at the fame time, that very accurate obfervations demonftrate, that fome worms, if not of the human body, at leaft of the bodies of particular animals, are produced by infects of the great world. Such are thofe inhabiting the rectum of the horfe, the frontal finus of fheep, goats, and the larynx of ftags, as the eminent and moft expert naturalifts, Vallifneri and Reaumur, have difcovered.

With refpect to feminal vermiculi, my obfervations will not allow me to afcribe them to an external origin; was it fo, I muft certainly have once perceived it. More than fourteen years have elapfed fince I have been occupied with infufions, with ftudying the waters of marfhes, ponds, and ditches, for thefe are generally full of microfcopic animalcula; and, with abfolute fincerity, I can affirm, that among the innumerable minute animals contemplated by me, never was one feen refembling the feminal vermiculi of man and quadrupeds (1). I do not deny that, admitting

(1) This had already been remarked by Leeuwenhoeck, as appears from his 301 letter. 'Licet varias, et indole 'diverfiffimas aquas contemplatus fim, nec iftiufmodi ani-'malcula, (id eft fpermatica), nec quidquam quod ani-'malcula ifta fimilitudine aliqua, vel figura referret, in ullis 'unquam aquis obfervaverim.' Other obfervers agree with Leeuwenhoeck and myfelf as much as could be defired, Among 146 genera which Muller has claffed, he has feen only

admitting they pass from water to animated bodies, they may possibly undergo some change or alteration, operated perhaps by the causes the Genevese philosopher details, which has been rendered probable by demonstrating, that animals which change their climate and aliment suffer such mutations, 'Ranæ in Ebusum Insulam delatæ colores mutant; Oves in regione septentrionali albescunt, in meridionali nigrescunt. Sic Vulpes, Ursi, Lepores mutato loco, colores et quandoque mores mutant.' I likewise acknowledge that the body of vermiculi may change its proportions; that the parts may become larger or smaller according as the new element agrees with their nature; as also happens in plants carried to a foreign soil; but I cannot think they will lose their pristine figure to assume

only one, (which he calls Cercaria), resembling the seminal vermiculi of the ram. But this species, which is the only one that has been observed, and is very seldom found, (in infusione animali raro), would be far from being able to give existence to all the different species of vermiculi.—A.

Muller makes this remark on the *Cercaria Gyrinus*. There are others of the same genus that may resemble seminal vermiculi; and perhaps some of the Trichodæ. But of the many hundred figures in his *Animalcula Infusoria*, a work that no animalculist can want, certainly there are none that may be confounded with the vermiculi: Many, however, have tails unconnected with the motions of the body.—T.

fume one very different, or that the first change will be such as to prevent them being recognised, for then the internal structure must also be changed; and this is as much as to say, that after their former organs were wholly or partially changed, they should produce new ones, which would rather be a creation than a change.

The seminal vermiculi not only differ from animalcula in shape, but I have likewise shewn that they are of a nature and constitution essentially different. Such are the differences mentioned in the former chapter and that preceding it, but omitted here to avoid repetition.

M. Bonnet suggests a most ingenious experiment, to try whether seminal vermiculi will live in infusions, and infusion animalcula in seminal fluids. I had already done this in part; but, on transmitting the animalcula of putrid into recent semen, and the vermiculi of recent semen into that which was putrid, the whole animalcula and vermiculi perished. Yet, to satisfy the curiosity of my illustrious friend, instead of corrupted semen, I used vegetable infusions, and took the precaution to make them of the same heat as semen in the body of the animal, and that the semen where the animalcula were put should have the temperature of the atmosphere; however their death could never be prevented. There was a little difference: the vermiculi perished

periſhed immediately; the animalcula in a few minutes.

The great difference of aliment to which vermiculi muſt be accuſtomed, on paſſing from without into the ſemen of animals, ſeems to me a ſufficient reaſon why that cannot be their origin. For the ſmall animals of our world periſh when obliged to change their food, as we ſee in caterpillars feeding on ſpecific plants; when the plants are changed they die: ſo that, if we give ſilk worms any other than mulberry leaves they very ſoon ſuffer. That inſects ſhould die, it is not neceſſary the plants ſhould be changed, even a change of the parts that they naturally occupy is ſufficient. Let us conſider thoſe whoſe common abode is on the ſame tree; and how many hundred ſpecies inhabit the pear? Some feed and dwell on the ligneous parts; ſome inſinuate themſelves between the bark and the wood, and never leave them; ſome conceal themſelves by folding ſeveral leaves together, and feed on the moſt delicate parts; others prefer the roots, and, by penetrating, form tumours there; ſome menace the flowers, and others the fruit. Let us change this order; let us tranſport the inſects of the wood into the bark, and reciprocally; thoſe of the leaves to the roots, and ſo on with the reſt; it will not be long before they die. I cannot conceive why the ſame ſhould not happen to ſpermatic

spermatic vermiculi on passing from without to the semen, since their aliment is completely changed. It is not enough to say, we have an example in the larvæ found in the horse, the sheep, or the stag, which live in a place where their existence did not begin; for I may answer, they have not passed from a large to a little world after living in the former, but are developed in the quadrupeds where they have been deposited by their parent flies, where they remain until maturity, and feed on the substance of the animal. If, before acquiring this maturity, their situation was fortuitously changed, it is most likely they would perish. We should see them perish, or rather not expand, if the flies laid their eggs elsewhere. Whence it follows, that this example confirms the general law.

If we cannot believe that vermiculi come from without, what is their origin? We shall answer what Vallisneri has said on the origin of the large worms in the human body.—They are produced, nourished, and multiplied in us and other animals:—they pass from generation to generation with the nutriment the mother affords in the uterus, or with the milk that is imbibed (1). This hypothesis seems to me much more probable than the other. According to M. De Buffon, the semen

(1) Vallisneri, T. 2, edition in folio.

men of the female is full of vermiculi perfectly like thofe of the male. I do not doubt that it is truly fo. The fame had been before obferved and defcribed by Signor Bono, a celebrated phyfician and an exellent obferver of fpermatic vermiculi, incapable of altering any fact, as he was unprejudiced in favour of theory. What has been remarked, by thefe two naturalifts, I have fometimes, but rarely, feen in the blood. In my long remarked on the phenomena of circulation, I happened to obferve, in the mefenteric blood of a frog and three newts, I know not how many of the feminal vermiculi peculiar to thefe amphibia. There was no hazard of deception, becaufe there was no room for error. One cannot think, thefe vermiculi were mixed with the blood, from the rupture of fome of the blood veffels of the tefticles or vafa deferentia, fince the frog and two of the newts were females; and the blood veffels, as well as the generative organs of the male, were entire, as I affured myfelf by a careful examination. The vermiculi were actually confined in the veffels, and were moft vivacious. They appeared in the arteries, excepting a fingle time that I faw them in a vein. The artery of a frog tadpole fhewed me fome again: fome were even feen in the blood, ftill warm, of a fucking calf; and feveral among the red globules in the blood of a ram. I could not but recognife them,

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as they had all the characteristics of the seminal vermiculi, peculiar to the two species of animals. These observations prevented my surprise, when mixing a drop of semen with a drop of blood, so that the vermiculi were forced from their proper liquid into the blood, they still lived as before. The same has taken place with saliva; and it is natural to suppose it will happen with other animal fluids.

From these facts I draw two conclusions; *first*, That it is not absurd to suppose that the mother may serve as a vehicle for the seminal vermiculi to transmit them to the young; *secondly*, That they live in the fluids of the young, particularly the blood, and are, *in a manner*, retained there until puberty. I say, *in a manner*, because the rareness of seminal vermiculi in the blood sufficiently proves it is not a fluid that agrees too well with them; perhaps, because the aliment found there is not very well adapted to their nature; and although the semen is derived from the blood, they are two very different fluids. To this it may be opposed, that the vermiculi found in the blood of the male are re-absorbed by the vessels, and mixed with the mass of the blood. The objection would be well founded if applied only to adult males; but it is insufficient where they are not so, as tadpoles and sucking calves. The organs of generation are not unfolded in the

the former; and thofe of the latter are not inhabited by vermiculi.

Befides the mother, the father may be an inftrumental caufe in the propagation of vermiculi: which may take place with almoft every fpecies of animal. This method is at leaft more direct, or perhaps more natural than the other: I fpeak of the act of fecundation, which may convey the germs of the vermiculi to the embryo by the immediate vehicle of the femen. That the eggs of females may be impregnated, they muft be bedewed by the fpermatic fluid of the male, which muft act on the included embryo: it fhould act not only externally, but alfo internally: for we know that it regulates the parts of the fœtus (1). Therefore it is neceffary that the fœtus fhould be penetrated by it; and in this way may the vermicular germs be eafily introduced. Thefe expand or lay the foundation of a little colony of vermiculi, which take poffeffion of the feminal fluid, and give birth to numerous inhabitants.

The fecond queftion relates to the mode in which vermiculi propagate. In the number of experiments, almoft infinite, that I have made, my attention has always been directed to this interefting point. After feeing a prodigious multitude of infufion animalcula multiply by divifion of the body,

(1) Bonnet, Pref. a la Contemplation.

body, I investigated whether seminal vermiculi propagated in the same manner, but there has been no indication of this. It is true, when they pass from the animal's body, or when the vermiculi begin to be in a morbid state, they are less fit for dividing than when in their natural state in the seminal reservoirs when vigorous and full of life. I do not deny that this is possible; but admitting it was so, it seems morally impossible that, among so many millions of vermiculi, which I have at different times observed: among so many species, there was not one in a state to divide, or which did divide, as is seen with the animalcula of infusions. Neither have I observed that vermiculi propagate by buds or shoots, like polypi. Therefore, abiding by the different modes of propagation hitherto known, it would appear a proper conclusion, that seminal vermiculi multiply by means of a fœtus, or of eggs; but I must admit, that neither the one nor the other has been seen by me.

I come to the last question. What is the use of the vermiculi? Leeuwenhoeck's opinion is well known: He thinks they are the immediate authors of generation; so that those of man will be so many homunculi; those of the bull, so many vituli; those of the horse, so many foals; and thus of the rest. We cannot deny the idea is very ingenious: it is unfortunate that it is not real.

real. I ſhould deviate from my plan, by attempting to refute it. Authors, celebrated and known to natural philoſophers, have done ſo with ſucceſs. But I cannot reſiſt ſaying a word on the beautiful diſcovery of Haller, which is completely deciſive. He proves, by facts ſo convincing that it is impoſſible to withhold our aſſent, that the fœtus belongs to the female; that entire, it pre-exiſts fecundation (1). It is evident, therefore, that the vermiculi afforded by the male cannot be fœtuſes. The facts eſtabliſhing this diſcovery are explained in his excellent treatiſe on the chicken.

Though we refuſe the vermiculi this noble purpoſe, we do not fail to aſcribe others to them. Some phyſiologiſts have ſuppoſed them the cauſe of venereal pleaſures; others, that they preſerve the fluidity of the ſemen; and ſome, that they effect certain unknown purpoſes. All have endeavoured to divine their uſe.—There exiſts in us, and in other animals, this wonderful multitude of living beings, and many larger vermes, of which

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(1) The author diſcovered that the tadpoles of frogs, toads, and newts, pre-exiſted fecundation; that they exiſted in the egg produced by the mother, before it was moiſtened by the ſemen of the male, *Diſſertations on Animals and Vegetables*. He alſo diſcovered, that, in the egg of the torpedo, a fœtus exiſts previous to fecundation, *Lettera Sopra Diverſe Produzione Marine*.—T.

the eminent Redi has given a hiſtory in a work on the ſubject (1). Beginning with man, and deſcending to animals, not excepting the ſmalleſt, each in like manner has external inſects which it feeds, as the ſame naturaliſt proves in another treatiſe (2). But why do all theſe races of animals inhabit the internal and external parts of others? Why were they created? This, I think, is beyond the ſphere of human knowledge; and it will not diſpleaſe, if here I ſhould be ſilent. And I hope I ſhall be pardoned for only gueſſing at the other two; their intricacy has prevented me from doing more.

The intent of this tract was to examine, with all poſſible attention and accuracy, the properties and nature of the myſterious inhabitants of animal ſeminal fluids; and I have taken the liberty to diſcuſs and refute the opinion of others, learned in the ſubject, becauſe their celebrity and diviſion had created doubt. The intelligent, judicious, and impartial reader is left to conſider whether I have ſucceeded, and whether I have diſſipated, at leaſt diminiſhed, the obſcurity that veiled them from the truth.

(1) Degli animali viventi negli animali viventi.

(2) Eſperienze intorno agli inſetti.

OBSER-

OBSERVATIONS AND EXPERIMENTS

ON

ANIMALS AND VEGETABLES CONFINED IN STAGNANT AIR.

INTRODUCTION.

If privation of air is a powerful method of preventing the production of animated beings, and of destroying life in those already in existence, the presence of air itself, when its circulation is impeded, is considered equally noxious. A general rule has been established, that all animals and vegetables forced to respire the air of close vessels perish irrecoverably. It is thought equally certain, that seeds do not germinate, and eggs are not excluded in this situation. The great Boerhaave speaks thus of all. "Ovula quorumcumque insectorum in vitris accurate clausis "non producunt, licet tepore fota, fœtus; semina "plantarum vite macerata, optimæ commissa ter-

"ræ, atque requiſito excitata calore, non tamen
"creſcunt, neque dant vitæ ulla ſigna actuo-
"ſæ (1)."

Such is the received opinion of all philoſophers and naturaliſts, which for many years I confidently adopted, and believed certain with reſpect to the animal and vegetable kingdoms. But my experiments on infuſions inſpired me with a juſt diſtruſt. I had diſcovered that the animalcula of infuſions were produced, and lived, in veſſels hermetically ſealed; and that the ſeeds uſed for the infuſions germinated. Theſe facts did not correſpond with the general belief; and excited a lively deſire of inſtituting a number of experiments to inveſtigate the limits and conditions of the phyſical maxim, That air which is confined and cannot be renewed is noxious to beings in life, whether animals or vegetables. With this deſign, I reſolved to repeat and to diverſify my experiments on infuſion animalcula and vegetable ſeeds growing in cloſe veſſels; and alſo to make experiments on ſome animals, whoſe great analogy to infuſion animalcula would induce us to ſuppoſe that the noxious influence of ſtagnant air could not ſo eaſily affect them as it would affect animals ranking higher in the ſcale of beings. My experiments were therefore made on the eggs of many terreſtrial and aquatic inſects;

(1) Elem. Chem.

fects; and nature has afforded those illustrations which former authors have sought in vain.

From effects I ascended to causes, and investigated why confined air could, in certain situations, be injurious to animated beings. Thus, passing from one research to another, the work insensibly increased in my hands, and became much more considerable than I expected. Since I now presume to publish it, that the tedium of the reader may be lessened, it is divided into three sections or chapters. The principal objects of the two last are researches on the cause of animals dying in confined air; and the first comprehends a view of those beings that are subject to its influence only in certain circumstances.

CHAP. I.

INFUSIONS, VEGETABLE SEEDS, ANIMALS EGGS, AND ANIMALS THEMSELVES, SUBJECTED TO STAGNANT AIR.

I PROVIDED a certain number of vessels: In each I put an infusion of vegetable seeds, and then sealed them hermetically. They were made exprefsly for the purpose, large, and each might

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contain fourteen or fifteen pounds of water (1). It was not neceſſary to break the veſſels for examining the infuſions, but ſufficient to cauſe the included liquid flow to a dry part of the veſſel; and, after it had flowed back, to obſerve the part that had been wet, with a powerful magnifier. The glaſs was ſo tranſparent, that animalcula, if any were produced, could be ſeen ſwimming in the thin pellicle adhering to the internal ſurface of the vaſe.

My experiments were made towards the end of ſpring; and it was not long before animalcula began to inhabit the veſſels, and inhabit the whole. The periods of their increaſe, diminution, and deſtruction, were the ſame as in the open air.

On repeating the ſame experiment often with different ſeeds, all afforded animalcula. One difference was to be remarked. The number was never ſo prodigiouſly great which I have obſerved before. One mode by which theſe animals propagate is, the natural diviſion of the body; and this diviſion alſo ſucceeds in confined air. Applying the magnifier to the ſide of the veſſel, I ſometimes ſaw them dividing through the middle; ſo that the parts were connected by a ſhort filament; others appeared like two minute elongated ſpheres,

(1) Twelve ounces in the pound.

ſpheres, touching in ſeveral points; others exhibited a contraction, or rudiments of diviſion hardly begun, on the outſide of the body.

The duration of life, and the multiplication obſerved in infuſion animalcula, were likewiſe ſeen in the anguillae of vinegar confined in cloſe veſſels. From the beginning of April to the end of November, they were viſible, and continually became more numerous. It is true, as winter advanced, the eels periſhed; but the ſame happened to thoſe of vinegar in the open air, which was occaſioned by the increaſed cold. We know that vinegar is without eels during winter.

While I made theſe experiments, the water of ſeveral ditches was full of worms, inſects, and the tadpoles of frogs. On them were repeated the ſame experiments as had been made on the animalcula of infuſions and the eels of vinegar. I began with the larvæ of muſkitoes. Many were confined in veſſels with ditch water, that they might find aliment among the quantity of heterogeneous matter which it was full of. The larvæ ſuffered nothing from confinement: all changed to nymphs, which in time produced the flies.

Tadpoles were alſo confined in veſſels, along with a ſufficient quantity of water, and marſh lentil for food. For twenty-four days, their ſize encreaſed conſiderably; and they died leſs from ſtagnation of the air, it is probable, than be-

 cauſe

caufe the lentil was completely confumed. The tadpoles were young: I repeated the experiment on grown tadpoles, whofe limbs had already begun to appear; chufing this period to learn whether they underwent their metamorphofis in ftagnant air. Several did undergo it, and divefted themfelves of the tadpole form to affume that of the frog; but others perifhed before attaining their new ftate.

Animals inhabiting the waters are not under the fame neceffity of continually refpiring air, as thofe deftined by nature to live amidft the air itfelf: therefore it was fit to make fome experiments on the latter. And having made feveral on tadpoles, that is, on a fpecies of animals changing their ftate, I was defirous to repeat them on other animals of the fame nature. Caterpillars immediately occurred; and my firft experiments were on filk worms. Thofe that were taken would in a few days have begun to fpin their web. Leaving them on a mulberry branch, the ligneous extremity of which was immerfed in water, that the verdure of the leaves might be fome time preferved and ferve for food, I fealed them hermetically in a veffel. Above a third perifhed; but the reft, being eleven in number, proceeded as ufual; they worked the accuftomed web, fixed to the fide of the veffel, and there fhut themfelves up. Butterflies came from

from nine webs: from two, there came none. On examining the ſterile webs, I found the caterpillar transformed to a chryſalis; but the butterfly did not come out, as it died while in this ſtate. The webs of the eleven caterpillars in this cloſe veſſel were of good ſilk; and their only difference from others was in not being ſo hard and elaſtic as cocons generally are.

Other caterpillars, eſpecially thoſe of the elm and oak, underwent changes ſimilar to the ſilkworms in cloſe veſſels. The ſame method was adopted of confining them in veſſels where the lower extremities of branches had been immerſed in water.

The metamorphoſes of the larvæ of large fleſh flies were more diſtinctly ſeen. I put a piece of fleſh nearly putrid at the bottom of one of the veſſels: it ſerved them for food nine days, that is, all the time they were worms, and until they became nymphs. We know, that when their change approaches, they abandon the putrid fleſh, and, ſeeking a dry ſituation, moſt commonly conceal themſelves under an arid duſty earth. Thoſe confined likewiſe abandoned the fleſh, traverſed the veſſel, and were in conſtant motion more than half a day. Their anxiety to eſcape was evident; but, unable to effect it, they retired to the ſides at the extremity of the neck of the veſſel, which was almoſt parallel with the floor, and became

became perfectly tranquil. There they insensibly contracted; their shape and colour disappeared; they assumed a light chesnut shade; and exhibited every sign of being changed to nymphs. In this state, they remained fourteen days; then, the shell bursting, flies completely resembling the parent fly escaped. The winged insects lived several days in their prison, and died apparently for want of food.

A few words may be said concerning the seeds used for infusions. They developed like the animalcula; all germinated well; and in a few days, the budding and branching of the leaves filled the capacity of my vessels. I should not omit observing, that the whole of these vegetations shewed symptoms of disease, whether from decaying before fructification or from their yellowish colour. Suspecting that this disease was not so much the effect of privation of circulating air as of the beneficent influence of the sun, and the moisture that the roots must supply, which could not be obtained from the small quantity of water, I endeavoured to ascertain the real cause. The same quantity of seeds was put into vessels hermetically sealed, and, instead of water, I substituted a portion of well moistened earth. The plants soon sprung up; and, being exposed in the sun beams some hours of the day, in a short time, reached the summit of the vessel without becoming

coming yellow till after a considerable interval. The seeds were pease, maize, red wheat, and rye. I planted two stalks of rye under the neck of the vessel, where there was a sufficient space for their elevation: the neck being very long, they exhibited unequivocal marks of incipient fructification by the ear shooting out of the calyx; and it would have made the greatest progress but for the approach of winter.

To complete what has been said of seeds confined in vessels, it may be remarked, that I have in this way observed more than twenty species at different times without finding one that did not germinate. A precaution essential to their production must be attended to: whenever they spring in water, which takes place in close as well as in open vessels, the infused seeds must be partially above the water, otherwise they perish: which was adverted to before me by the celebrated naturalist M. Duhamel.

If vegetable seeds germinate without exception in confined air, what are we to think of animal semina, or the eggs of insects, which, according to Boerhaave, and the general opinion of philosophers, should remain sterile, even when the operation of circumstances the most favourable to their production concurs? Here I thought it better to consult nature than to trust to the sentiments of others. I therefore made experiments on many eggs: on those of beetles, flies, flesh

flies,

flies, nocturnal and diurnal butterflies, worms, and others, and scrupulously observed what happened to each kind. I foresee the reader's anxiety to learn the result of these experiments; and in two words his curiosity may be satisfied, by learning that the whole different species were excluded equally in confined as in open air. Boerhaave, adopting the received maxim concerning the sterility of eggs in confined air, thus expresses himself in his Prælectiones Academicæ (1). 'Ova bombycis in aere calido excluduntur, si libere admittatur. Eadem in phiala clausa nunquam producunt suum animal.' Now, the truth is, these hatch very well in close vessels, as I have been convinced by every experiment I made. From all that has been said, we must conclude, that the air of close vessels is not an impediment to the production of plants or animals; but plants, without any exception known to me, germinate there, and animals grow and propagate their species. Those which undergo metamorphoses experience the changes in close equally as in open vessels.

Why do we generally believe that stagnant air obstructs the production of animals and vegetables? Analogy is surely the cause of this remarkable error. It was observed, that animals and vegetables subjected to experiment soon perished

(1) Tom. 2.

perifhed in clofe veffels; that feeds and eggs confined were fterile; and a general conclufion was made, that ftagnant air was decidedly noxious to both kinds of living beings.

In the beginning of this chapter, it is faid, the veffels employed were very large; that each might contain fourteen or fifteen pounds of water; with them the refults enumerated were obtained. But the confequences are very different on ufing veffels fucceffively fmaller: In proportion as their capacity decreafes, the eggs or feeds do not germinate, or they perifh when fcarcely evolved, and animals of every fpecies in a fhort time die. All the naturalifts, who have had refults different from mine, certainly have ufed veffels too fmall, otherwife the fame would have happened as to myfelf. However, I do not deny that their error, refpecting animals, might arife from the nature of the animals on which their experiments were made, that they muft perifh whatever the capacity of the veffels may be, as, for inftance, warm blooded animals. But to underftand this better, and enforce conviction, let us defcend to particulars.

Animalcula of infufions originate, exift, and propagate in the large veffels; which alfo fucceeds in veffels one third of the fize, only we then begin to perceive the difadvantages of ftagnant air. When the capacity is fuch as to contain three

three pounds and a half of water, the number is leſs; they multiply leſs, and die ſooner. On diminiſhing the veſſels, the larger animalcula are not ſeen, and neither large nor ſmall, if the internal capacity does not exceed ſeven or eight inches.

The nymphs of gnats ſeem to ſupport this ſituation better than infuſion animalcula. In five inches of air, many changed to the winged ſtate. As the quantity leſſens, they periſh proportionally ſooner, and without changing.

The eels of white vinegar are particularly remarkable. They live and multiply prodigiouſly, where the volume of air does not exceed three inches; and die in ſeveral days only, when confined in a tube where the vacuum is leſs than an inch. I ſpeak of white vinegar, for the effects are very different with red. In my experiments, the eels of the latter did not live five days in a veſſel where the vacuum was eleven inches. This did not happen ſo much becauſe the vinegar underwent an alteration, as that the eels of red vinegar rather are of a different nature from thoſe of white; which I believe to be more probable, from the difference of figure I remarked in each ſpecies.

Tadpoles periſhed in ſome days in nine inches of air; and in a few hours, if the vacuum was but three inches.

Caterpillars,

Caterpillars, and the larvæ of flesh-flies, confined in eleven inches of air, died before becoming chrysalids. The larvæ in particular, soon after confinement, deserted the putrid flesh put in along with them for food, and tumultuously traversed the vessel, disregarding the flesh. They became motionless, and died after various periods, longer or shorter: if the vessel was larger, they lived longer; if smaller, they died sooner. These larvæ, when changed to nymphs, suffered less from the small quantity of air. In a vessel where the larvæ had died, I confined the same number of their nymphs. The flies of some came out; but it must be remarked, that the body and wings were distorted: they seemed to have been produced, as one may say, against the will of nature. This never happened to the chrysalids of butterflies, though the vacuum was very small.

What has been hitherto said will apply to seeds and eggs. I omit telling the reader my trouble in finding the successive capacities of the vessels, where eggs and seeds ceased to germinate; but adopting the general result, when the capacity of the vessels was but three or four inches, neither eggs nor seeds have developed.

Thus it is necessary to conclude, that the production of vegetables, and of some animals, takes place nearly as well in confined as in open air, provided the quantity in the vessels is considerable;

able; on the contrary, when it is not, that it becomes fatal to both. The precife quantity which may be noxious can only be determined by the nature, conftitution, and quality of the animals and vegetables confined.

By experiments in different feafons, I difcovered that the death of animals is not only accelerated by diminifhing the fize of the veffels, as I have fhewn, but alfo by the increafe of heat. This is particularly feen in animals which are eafily procured at any time of the year, and live long without food; fuch as, newts, leeches, land and water ferpents, vipers, and fome fpecies of fifhes. I was as careful as poffible to take the individuals, for my experiments, of an equal fize and equally vigorous, fo that the comparifons might be the more juft. While engaged with very different matters, I difcovered this new fact in the following manner. On the fifth of April, along with other things, I prepared three jars; the firft might contain fix pounds of water; the fecond, four; and the third, two. Four newts were confined in each. My experiments were directed to inveftigate whether the animals died fooner as the volume of air diminifhed; and I found it to be the cafe. The four newts in the fmalleft veffel perifhed in forty-one hours; in the intermediate veffel, in two days; and in the largeft, in feven days.

On

On the ſame day, a ſimilar experiment was made with leeches. I confined four in each veſſel. They lived long in compariſon to the newts. In the ſmalleſt veſſel, they died in three days; in the next, in nine; and in the largeſt, in thirty-two days.

The experiment was repeated, 12 May, on both kinds of animals. They died much ſooner. The newts, in the ſmalleſt veſſel, periſhed in twenty-ſeven hours; in the next, in three days; and in the largeſt, in four days. The leeches, in the ſmalleſt veſſel, died in two days; in the next, in five; and in the largeſt, in nine. The more immediate death of the newts and leeches in May, I ſuſpected was occaſioned by the increaſed heat of the ſeaſon; for, during the month of April, in the greateſt heat, the thermometer roſe to 57°, while, in May, it ſtood at 70°. My ſuſpicions were realiſed by the death of the animals being accelerated in June and July. The thermometer ſtanding at 81°, during the latter, four newts in the largeſt veſſel died in twenty-three hours, and the leeches in thirty-five.

What has been ſaid of leeches and newts will apply to ſnakes, vipers, and fiſhes; becauſe the reſult of experiments made on them correſponded with the former. The death of the whole was not only accelerated in proportion to the ſmall quantity of air they were forced to reſpire, but alſo in proportion to the increaſe of heat. Twice I obſerved the reverſe, which we muſt conſider as ariſing from ſome accidental circumſtance.

I delayed until winter to make the experiment inverſely; that is, to learn whether the death of animals was retarded in proportion to the increaſe of cold. The experiment ſucceeded with vipers and newts, which were the animals I then had at command. The newts lived twenty-two days in the ſmalleſt veſſel; in the middle-ſized, thirty-four; and in the largeſt, two months. Vipers ſurvived ſtill longer. The veſſels were in a ſituation where the thermometer ſtood at 48°.

Life was protracted in a greater degree of cold. When the veſſels were kept under ſnow, or, which is the ſame, at freezing, the animals did not periſh in three months. On taking them from the ſnow, and expoſing them ſome days to the atmoſphere, during ſpring, both kinds died.

Such are the principal reſults which I have been able to collect from the experiments related in this chapter; reſults moſt uſeful in themſelves, becauſe they elucidate the ſubject: but they leave us to ſuppoſe the cauſe, or rather induce us to ſeek for it; which the reader will endeavour to diſcover, if he is a philoſopher. Why is the death of animals accelerated in ſmall veſſels, and retarded in large? Why is it accelerated more by by heat than cold? Whence arises the difference of time in the death of animals? Why may one quantity of air be noxious to one species of animals and indifferent to another? The solution of these problems depends on our knowledge of the cause of death in stagnant air. This ancient and most famous question has always divided celebrated modern philosophers. It is important to enter on the discussion of it; and I shall examine what has been already written on the subject, and adopt that opinion which to me seems most consistent with facts, that is, with truth. Since the eggs of animals, and seeds of plants, in a small quantity of air, remain sterile, I shall not fail to add a short sentence or two on the cause of their sterility.

CHAP. II.

TWO PRINCIPAL OPINIONS ON THE CAUSE OF ANIMALS DYING IN STAGNANT AIR.—WHETHER IT LIES IN THE DIMINISHED ELASTICITY OF THE AIR.

Two phenomena have been remarked by those who have killed animals in close vessels; *first*, That a quantity of vapour, exhaled from the animal, accumulates on the ſides of the veſſels; *ſecondly*, That the air has loſt a certain degree of its elaſticity. Theſe phenomena have produced different opinions. One aſcribes the death of animals to theſe exhalations, which, being confined in the veſſels, are reſpired by the animals, and thus become fatal. Another opinion maintains, that the exhalations cannot be mortal; but the elaſticity of the air being diminiſhed by them, or a portion of the air being deſtroyed by reſpiration, becomes fatal.

The experiment of Piſtorini of Bologna, inſtituted to appreciate the force of both opinions, is ſpecious. Suppoſing they are equally juſt, he argues, it ſhould neceſſarily enſue that two animals, confined in the ſame veſſel, die ſooner than one alone, provided the veſſel is the ſame, and the animals of the ſame ſize and ſpecies. We muſt, therefore, recur to the exhalations from the animal, or to diminiſhed elaſticity of the air occaſioned by the effluvia or the reſpiration; and as it is always certain, that doubling the number of animals, the exhalations and reſpirations are doubled, the diminution of elaſticity ſhould conſequently be doubled. But Piſtorini found it otherwiſe. Two animals died as ſoon as one, though he uſed the ſame veſſel, and animals of the ſame ſize and ſpecies (1).

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(1) Act. Bon. tom. 2, part 1.

It was to be ſuppoſed, that the ſingular conſequences of the experiment induced others to repeat it; among whom was the celebrated Profeſſor Veratti. His experiments were made on birds and frogs. A pigeon lived three hours and three quarters: two pigeons, confined together, lived only half the time. Three ſwallows died in little more than half an hour; two ſwallows in leſs than an hour; and one lived almoſt two hours. He obſerved nearly the ſame with ſparrows and quails; three died ſooner than two, and two ſooner than one. But with frogs it was altogether different: in eight days, four died as ſoon as two, and one alone lived no longer than three, though the veſſel was always the ſame. So that the experiments on birds were very different from thoſe of Piſtorini; and the experiments on frogs agreed with his. And here is ſeen the different nature of animals in this mode of death. Thus far Sig. Veratti (1).

Sig. Cigna, a profeſſor equally celebrated, has alſo engaged in an examination of theſe diſcrepancies, which he thinks to have removed by the accuracy of his experiments. The reſult is this: Where the frogs confined are deprived of water, as it would appear thoſe of Veratti were, there it is true that the plurality never or ſeldom accelerates death. When confined along with water,

 which

(1) T. cit.

which is their natural aliment, it is almoſt certain that the acceleration of death is in proportion to their numbers (1).

The experiments related in the preceding chapter induced me likewiſe to engage in this inquiry. If it was true that ſeveral animals of a given ſpecies confined in the ſame veſſel died as ſoon as where there was only one, and if the phenomena was not accidental but conſtant, it muſt be regulated, as I ſaid before, in a manner proportioned to the ſmaller quantity of air confined along with the animals. But we may eaſily ſee, when the veſſels are equal, that the ſmalleſt quantity muſt always be where there are moſt animals. As frogs had diſturbed the order eſtabliſhed by this law, according to Veratti, and as I could not repeat the experiments of Piſtorini, becauſe he has not ſpecified the animals he employed, my experiments were on frogs alſo; ſome in veſſels with water, and ſome in veſſels without it, thus to approach the method followed by theſe authors. In three veſſels, which would each contain five pounds of water, I hermetically ſealed up frogs, that is, two in one, four in another, and eight in a third. In this laſt, the eight frogs periſhed in twenty-ſix hours; in the ſecond, the four frogs died in one day; and in the other, the two periſhed in two days.

A ſimilar experiment was at the ſame time made with three veſſels more, as large as the firſt,

(1) Miſcell. Taur. Tom. 2.

firſt, and the frogs diſtributed in the ſame manner: ſo that there was no difference between this and the preceding experiment, except that there was no water in the veſſels; and here there were four ounces in each. In two days, none of the frogs were alive in the veſſel with eight; in that with four, they lived three days and a half, and five days where two were. During theſe experiments, the thermometer ſtood between 63° and 70°.

Both experiments were repeated, the circumſtances in every reſpect the ſame, except that the heat of the weather was greater; and then the thermometer aſcended to 77°. The frogs in the firſt veſſel died in twenty hours; in the ſecond, in nineteen; and the two frogs in the third, in about two days. As to the ſecond experiment with veſſels containing about four ounces of water; the eight frogs died in thirty-two hours, the four in two days, and the two in the third veſſel, in three days and a half.

The experiments were repeated ſeveral times; but, to avoid the tedium of dry details, I ſhall not circumſtantially deſcribe them, and will only ſpeak of the reſults obtained from veſſels without water. Sometimes I obſerved the irregularities which have already been remarked. It ſometimes happened, that more frogs periſhed in the ſame time, and ſometimes later than a ſmaller number. But when the frogs were in water, they conftantly perifhed fooner as their number was greater: eight died firft, then four, and laftly two. It happened only once that all the eight were alive, when one in the veffel with four was dead.

From all thefe facts, added to thofe related by Sig. Cigna, I was fatisfied that frogs corroborate the general rule, that all animals, without exception, perifh in confined air fooner according as their number is increafed. However, we fee difcrepancies with frogs included in veffels without water: but I know not whether they fhould really be confidered fuch, becaufe privation of water is injurious to thefe animals, which Sig. Cigna remarks. Frogs die in a fhort time in open veffels wanting water: therefore it is abfolutely neceffary to profcribe this as a caufe difturbing our experiments.

After finding the reafon why differences appeared with frogs, perhaps it would not be difficult to find it in Piftorini's animals, had he mentioned what they were, and the manner of conducting his experiments. With refpect to Sig. Veratti, we know that he generally ufed birds. But he has found, that they, as I have myfelf done, and fhall foon obferve, agree well with the rule laid down. Thus there is room to fufpect, that Piftorini's experiment has met with fome accident,

cident, without knowing in what that accident confifted, which rendered his refults different from thofe of others. It might be occafioned by the birds themfelves; perhaps that which he confined alone was lefs vigorous than thofe he confined together; whence all died in the fame time. Perhaps in his experiment with the two birds, all communication with the external air had not been prevented, which might eafily happen if the top of the veffel was covered only with leather, or any fubftance of a fimilar nature; or if, on inverting the mouth of the veffel, it had not been well fixed to the plane of pofition with maftic, glue, or the like. It is very poffible that fome invifible hole might remain, or fome opening, by which the air might get admiffion. To obviate all fufpicion of foreign air, it is neceffary to feal the veffel hermetically, or immerfe the mouth deep in water, as will be more clearly explained. In the preceding chapter, we have feen how much the heat of the weather accelerates the death of the animals confined. May we not fufpect that Piftorini made the experiment on a fingle bird in very warm weather, and on two in very cold: and that the death of the two birds was by this means retarded by the cold of the atmofphere, which would protract life as long as the fingle bird furvived?

Let

Let us leave this irregularity: but before refuming my principal object, it will be proper to glance at a doubt that occurred on feeing animals die fooner in a clofe than an open fituation, when the number was encreafed; and this was, whether more immediate death arofe from diminution of the volume of air only, or if the number would occafion it in another manner, and thus become a new caufe. To afcertain which was the cafe, I felected three equal veffels, and a certain number of the largeft frogs, as nearly of the fame fize as poffible. Two I confined in one veffel, along with a pound of water, and one put alone into each of the other two veffels, adding a quantity of water equal to the bulk of a frog: for difcovering which, I immerfed it in a veffel, and obferved the quantity overflowing. The quantities of air in the three veffels were equalized by this method, though in one were three frogs, and in each of the others only one. If the greater number of animals accelerated death abfolutely from diminifhing the volume of air, as the quantity was equal in all the three veffels, the four frogs fhould die in the fame time nearly. If numbers influenced the acceleration of death, the two in one veffel fhould die firft. I have faid nearly, for it would be very remarkable if the animals died exactly in the fame time. The two frogs in the firft veffel liv-

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ed two days; that in the ſecond died in three days and ſeventeen hours; and that in the third, died in three days and a half. This demonſtrates the influence of number in accelerating death, and other experiments confirm it beyond contradiction. I have repeated the ſame experiment five times, and obtained the ſame reſults. The two frogs in the firſt veſſel always died ſooner than thoſe in the ſecond and third, and the difference of time was very perceptible; it has uniformly been a day, ſometimes a day and a half, and ſometimes longer.

Inſtead of two frogs, I put three in the firſt veſſel, and only one in the reſt; but equalizing the volume of air, by adding a quantity of water equal to two frogs in bulk. The three frogs not only died ſooner than thoſe in the other two veſſels, but the difference was ſtill more conſpicuous than before: it was two days and one hour with the frogs in the ſecond; and two days and ſeven hours with the frogs in the third veſſel. By increaſing the number of frogs in the firſt veſſel, their death was conſtantly accelerated with reſpect to thoſe in the ſecond and third, though precautions were taken to equalize the volume of air by addition of water equal to the bulk of the frogs in the firſt veſſel.

I extended my experiments, and changed the ſubjects to ſeveral ſmall quadrupeds and birds. But

But notwithstanding the equal quantity of air in the vessels, I constantly found that they died sooner as they were more numerous; and that the acceleration of death was always in proportion to the encrease of number. Thus, it is evident, that the death of animals, in close vessels, invariably happen sooner as the animals are more numerous.

By what physical agent, by what means are they destroyed? Is it by their breath, or perhaps by injuring the quality of the air which they respire? Let us carefully examine these two hypotheses, beginning with that which is founded on an alteration of the state of the air. That it does lose part of its elasticity is evident from barometers put in the vessels where animals are confined. Stair observed that his fell an inch in one where a rat died. The descent varies. In Veratti's experiments, it sometimes fell eight lines, sometimes nine, twelve, or more, according to the nature of the animals. The experiments of Mayow, Boyle, Hales, and others, agree in establishing that a portion of the air is destroyed by animals confined in a close vessel. We cannot conclude from them, that the death of animals in close vessels is owing to the diminished elasticity of the air; at least there are no experiments positively proving the fact. It is first necessary to enquire, whether an alteration of the air always takes

takes place, when animals die in close vessels. Secondly, whether the degree of alteration is sufficient to kill the animals; for we know that every degree of diminished elasticity is not fatal to them.

Here Sig. Cigna has laboured in a manner that merits commendation. I shall afterwards employ some of his ideas. I have made a course of experiments, with the same view, which shall be abbreviated after relating the method adopted. Several air-pump glass receivers were inverted in a vessel of water. They opened and shut above by means of a metal stop-cock. The receiver being left open when immersed, a free passage was left for the internal air to escape above, in proportion as it was compressed by the rising water; thus the remanent portion preserved the natural degree of density as the external air; which was absolutely necessary for the accuracy of the experiment. This done, I closed the vessel, and, to make it more secure, passed several folds of leather round the stop-cock, to cut off all communication with the external air. I was certain that it could not insinuate itself, for the same receivers were used as in my pneumatic experiments. The animals being put in the receivers, the diminished elasticity could be seen by the ascent of the water within. If the animals were aquatic, or amphibious, I let them remain

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in water. If terreſtrial, they were put in a veſſel which was ſuſpended by a hook at the top of the receiver.

My firſt experiments were on frogs. Seven were confined in one receiver, and air left, equal in bulk to a pound of water. In half an hour, the water began to riſe above the level of that without, evidently proving that the elaſticity of the internal air was affected. The aſcent continued until all the frogs were dead or dying. The water had riſen eleven lines. I repeated the experiment with four frogs confined in the receiver. When the whole were dead, the water had aſcended ten lines. The elevation was one line higher in an experiment with two frogs. It was nine lines with only one frog.

I made ſimilar experiments on newts, reſerving the ſame quantity of air in the receiver as for frogs. The death of eight newts raiſed the water ſcarcely an inch; of four newts, nine lines; of two, ſix lines; and of one, five lines. The elevation of the water, therefore, diminiſhed with diminiſhing the number of newts.

By the death of eleven leeches, the water roſe five lines and a half, and by the death of three only one line.

Several naturaliſts have remarked how much ſmall animals injure the elaſticity of the air. I had alſo obſerved this in birds: Veratti's re-

ſearches

searches were chiefly directed to them: but nobody that I know of has made experiments on an animal partaking both of the nature of a bird and a quadruped, although it is not properly either the one or the other; I mean the bat; an animal so disgusting and forbidding in appearance, but at the same time as perfect as other animals, and the connection of quadrupeds with birds. Their ambiguous nature made me desirous of forcing them to respire the same air in close vessels: but it first occurred to try how long they could support a vacuum. How much sooner did they die than the cold blooded animals? Five bats, successively subjected to the experiment, did not live three minutes. They were of that species called, by M. D'Aubenton, the horse-shoe bat, from the circular line on the nose.

Though they died suddenly in vacuo, there was a limited proportion in confined air. Four, closed up in a vessel, lived scarcely an hour; two, an hour and a half; one, lived almost three hours. The water of the vessel, in the first case, ascended an inch and seven lines; in the second with two, an inch and three lines; and in that with one, eleven lines.

My experiments were extended to several reptiles; namely, vipers and some land snakes. Both these species having died in close vessels; the water rose to a certain degree as the number was greater.

The

The greateſt elevation occaſioned by the death of three vipers, was an inch and three lines; and the leaſt elevation, by the death of one viper, ſix lines. The death of one ſnake raiſed the water four lines; and of five ſnakes, an inch and ſeven lines.

The ſame is the caſe with ſmall lizards and fiſhes. The elevation of the water is in proportion to their numbers.

I have repeated all theſe experiments in every ſeaſon, and have uniformly ſeen the water within the receiver riſe; with this ſingle difference, that the elevation is more accelerated in warm than in cold weather, as well as the death of the animals ſubjected to experiment. I have alſo conſtantly obſerved, that elevation is ſo much the leſs as the animals are ſmaller. Vipers, ſnakes, and bats, raiſed it more than frogs, newts, and lizards; and lizards raiſed it more than leeches. This even takes place according to the difference of ſize in the ſame ſpecies. The death of a barbel, weighing a pound, raiſed the water above an inch: that of one weighing only two ounces, did not raiſe it two lines. It ſeemed to me, that, in conſequence of this proportion, the death of the ſmalleſt animals ſhould raiſe it very little, or hardly at all: which would happen if their death was not occaſioned by the diminiſhed elaſticity

elaſticity of the air; becauſe the water in the receivers, remaining at its original level, ſeemed to indicate that the elaſticity underwent no alteration. To diſcuſs this fact was moſt important.

I began with the larvæ of large fleſh flies. Thirty, extremely minute from being lately hatched, were put into a very ſmall receiver. I left them on the fleſh where they had been depoſited by the mother. They lived only ſeven hours in the receiver, and the water roſe half a line. The ſame experiment was repeated on fifteen more: I could ſcarcely perceive the water elevated above the level; and it undoubtedly was not when eight larvæ were taken, though the whole died.

The larvæ of common flies exhibited nearly the ſame phenomenon. The water roſe one third of a line, when the number was great; when ſmall, the riſe was not ſenſible.

The death of ſeven earth worms did not raiſe the water. The larvæ of nymphs and gnats had only an inch of air: they died in a day; and though ſome hundreds in number, the water ſtood at its original level in the receiver, after they died. The death of five rat-tailed worms did not ſenſibly alter the level; but the death of a greater number occaſioned a perceptible elevation.

Some ſtagnant waters are full of a kind of minute animals, called water lice, or fleas, by natu-

 raliſts.

ralifts. They are in conftant motion, darting through the water in various directions (1). Several thoufands lived two days and fome hours in a receiver, and died without any fenfible elevation of the water. Neither could I perceive any elevation of water full of animalcula, which died in two weeks.

My experiments on many infects which undergo no metamorphofes, as fpiders, fhell and naked fnails, millipedes, and on others that change their form, as caterpillars, chryfalids, and nymphs, demonftrated, that the death of a great number raifes the water a little, but not at all when they are few.

The facts we have obtained are now enough to decide on the object of our enquiry; efpecially if compared with thofe of Sig. Cigna and Veratti (2). Two principal refults arife: Firft, that

(1) The author evidently means various fpecies of *monoculi*—T.

(2) In the fourth volume of the *Acta Bononiæ* is a memoir concerning the effect of confined air, (in which a candle has burnt,) on animals, and fome other experiments on confined air. This memoir has probably been overlooked, though the *Acta* are frequently quoted. The author does not afcribe the death of animals 'to the diminifhed elafticity of the air, but to fome change which it 'undergoes from its natural ftate, by frequent infpiration 'and

that there are many animals whose death in close vessels does not lessen the elasticity of the air. Secondly, when it is diminished, it is very little. The first result is unfavourable to those who ascribe the death of animals to the diminished elasticity of the air. It cannot be attributed to this: for, if animals die, in many cases, without the air sensibly losing its elasticity, we must conclude that their death has another cause. And I doubt very much whether in any case diminished elasticity has occasioned death, because it was so little. By Veratti's experiments, it appears that the barometer did not fall much more than an inch at the greatest alteration. According to mine, the water in the receiver sometimes rose a few lines, sometimes near an inch, and at most an inch and seven lines; that is, one fourteenth of an inch, and seven fourteenths of a line of mercury. But we know that in changes of weather, there is a greater difference in the weight of the air. The mercury in the barometer sometimes falls more than an inch, and falls very fast, especially in storms, without affecting animals; otherwise,

'and expiration, so that the pabulum is consumed by the 'animal, or the air is decomposed, and becomes unfit to 'support life.' Thus, this author is very near the true theory of respired air. *Thomæ Laghii de Animalium in aere interclusorum interitu.*—T.

otherwise, neither cold nor warm blooded animals would live in safety on mountains where the mercury falls lower than in barometers placed in close vessels (1). Animals can not only live in air which has lost its elasticity to such a degree that the barometer falls some inches, as on the tops of lofty mountains, but in air, if it is renewed, where the barometer falls to less than half its natural height. Such is the ingenious experiment of Sig. Cigna. This acute experimentalist confined two sparrows in the receiver of an air-pump: one was left at liberty: the other put into a glass bottle, around the neck of which a very large empty bladder was tied. Then he began to exhaust the air until the mercury, which stood at twenty-seven inches and a half in the barometer, fell to nineteen. As much air was then returned to the receiver as depressed the mercury two inches within. In a short time afterwards, he drew the same quantity from the receiver,

(1) However, it is certain that animals removed from plains to mountains suffer considerably, though they may naturally live in safety on the summits of the highest. M. de Saussure ascended to the top of Mont Blanc; the barometer fell to $16\frac{1}{11}$ inches. His respiration was so much affected and he was enfeebled to such a degree, that with great difficulty he could buckle his shoe. *Senebier, Memoire sur la vie et les ecrits de Horace Benedict de Saussure.*—T.

receiver; which he foon returned, and continued this alternate exhauftion and return half an hour. The fparrows were always kept in air rarified fo much as to balance only eight inches and a half of mercury, or at moft ten and a half. But the fparrow at liberty enjoyed the benefit of refpiring renewed air; while that confined always refpired the fame. It expired foon after removal from the veffel, whereas the other came from the receiver in perfect health (1).

Boyle tells us that animals perifh in condenfed air rendered much more elaftic than the atmofpherical. I have often repeated the experiment, condenfing the air fometimes twice, thrice, or even more, than its natural ftate; and with him have obferved that the air, thus rendered moft elaftic, kills animals flower, but they perifh irrecoverably.

Thus it is experimentally demonftrated, that the diminifhed elafticity of the air is not, and cannot be, the efficient caufe of animals dying in clofe veffels. We have next to enquire whether their refpiration contributes towards their death; which is the hypothefis now to be difcuffed, and will be the fubject of the following chapter.

(1) Mifcell. T. 2.

CHAP. III.

WHETHER THE RESPIRATION OF ANIMALS IN STAGNANT AIR OCCASIONS THEIR DEATH.—WHY THE DEVELOPEMENT OF SEEDS AND EGGS IS, IN CERTAIN SITUATIONS, PREVENTED BY CONFINED AIR.

THREE points are to be examined in difcuffing the firft queftion; Whether the death of animals in confined air is occafioned by their refpiration? 1. If we actually find exhalations in clofe veffels where animals have died: 2. If the exhalations occafion their death: 3. Suppofing they are noxious, how do they operate?

With regard to the firft article, Sig. Cigna, the learned illuftrator of this theory, has proved the reality of the exhalations of refpiration, by the fœtid odour we are fenfible of on opening the veffels where animals have died, and a fort of vaporous pellicle covering the internal furface. In my experiments, I have almoft uniformly obferved this pellicle over the internal furface of the veffels where warm blooded animals, as birds, rats, or bats, have died, but never have remarked it with cold blooded animals. Something

thing indeed was perceptible on opening the vessels; and the odour was certainly foetid or cadaverous. I have been sensible of it in all my experiments, which are very numerous, even in those made on the smallest animals: so that the existence of this vapour is not to be doubted, though it is not always visible; either from the smallness of the quantity, or because it is of a dry nature which may prevent it from appearing under the form of an aqueous veil.

It is incontestible that these exhalations are the real cause of the death of animals. Sig. Cigna endeavours to prove it by recurring to the resemblance we remark between the phenomena exhibited by fluids evaporating in confined air and those seen in animals respiring in close vessels. The evaporation he has observed continues longer in proportion to the size of the vessels, and fills them sooner as the air is more rarified. Animals are also subject to these two conditions; they live longer as the quantity of confined air is greater, and perish sooner as the air becomes rarer (1).

Collecting some of the results hitherto given, and connecting them with those that I shall afterwards establish, it seems easy to prove this fact. We have seen that two circumstances accelerate

 the

(1) Lib. cit.

the death of animals: the increased heat of the atmosphere, and the number of animals confined! The exhalations are in these cases more copious: reason persuades us of it; and it is confirmed by the more foetid odour on opening the vessels. As we cannot ascribe the death of animals to the diminished elasticity of the air, and as no other causes appear, why may we not recur to the exhalations now become more dense and active? Besides, as we shall see, animals die sooner in close vessels where there are some already dead, because the exhalations are more abundant. By this means, we comprehend how animals confined alone, or in small numbers, survive much longer than where there are many, and why they live longer in cold weather, for in either case the quantity of the vapour is less. For the same reason, their lives will be abridged in proportion to the smallness of the vessels; as the vapours become denser from the little space they have to spread.

With all these facts, we cannot affirm that diminished elasticity of the air occasions death, and not the exhalations of respiration, or some material change which the air undergoes; particularly as, by continued inspiration and expiration, it will cease to be the same for want of free circulation, and will lose the *pabulum*, which is a substance or quality known only by name, but on on which every one makes the life of animals depend, either by being decompofed or corrupted, fo as to become unfit for refpiration. But the flender fupport of thefe reafons is demonftrated by animals themfelves, for they die in fituations where the communication of the internal and external air is uninterrupted. When feven frogs had died in a receiver, I opened the ftop-cock, and gave admiffion to the external air to balance that within; in an hour, other two frogs were confined, the ftop-cock remaining open, but they died in lefs than an hour and a half. Further, the frogs in the receiver perifhed fooner, as the number of dead frogs was greater, although the diameter of the opening above was at leaft two lines. Several birds, reptiles, and fmall quadrupeds had the fame fate when forced to remain among dead animals, though the receiver was open above. As the communication of the internal with the external air was uninterrupted, the imagined alteration or corruption of the air cannot exift, and death is certainly occafioned by the exhalations of refpiration, fince we have feen that it happens when the top of the receiver is open, as part of them can then efcape.

It may be remarked, in paffing, that the death of animals in an open receiver decifively proves that the diminifhed elafticity of the air does not

kill

kill them. I made a new experiment to ascertain positively whether they were destructive. The opening of the receiver was enlarged, and the exhalations were transmitted into a vessel applied to it. In the vessel were confined two swallows, and it was completely secured by a wooden stopper, well fitted. I preferred swallows as eight had previously died in the receiver. The influence of the exhalations was such, that both died in a quarter of an hour, though two confined in a similar vessel lived fully two hours.

The experiment was diversified, by collecting various quantities of vapour in the vessel. Animals have always lived in proportion to the quantity. Long after the vapours are confined, they retain their destructive property. Whatever animals have afforded them makes little difference in their influence: they are equally fatal to all others. Exhalations from birds kill quadrupeds, and those from quadrupeds are destructive of birds.

To terminate the investigation, I made the following experiment. Many animals having expired in a receiver during very warm weather, I opened the hole at the upper part, and presented a bird to the very fœtid vapour which escaped, so that it was forced to inspire the mephitic air; this and every one treated in the same manner perished.

Although

Although I conceive it impoſſible that any animal can live in confined air, if the veſſels are very ſmall, it is certain that ſome ſurvive much longer than others. Cold blooded animals generally exiſt longer than warm. In the ſame air where a newt or a frog will live a day, a ſparrow, a bat, or a rat, frequently do not live an hour. There is even a ſort of gradation among cold blooded animals. A newt lives longer than a frog, and a frog ſhorter than a leech. This alſo ſucceeds with inſects. In the opinion of ſome, it is not difficult to aſcribe a reaſon for the diverſity of theſe phenomena. Cold blooded animals are not only more tenacious of life, but they have incomparably leſs neceſſity to reſpire than the warm blooded have. How much longer will a frog, a viper, or a toad, ſurvive in vacuo than a bird? Not being under the ſame neceſſity to reſpire as the others, they will abſorb leſs of the exhalations in equal time. Why then is it wonderful if they live longer? Doubtleſs ſome analogous reaſon occaſions the difference of time that cold and warm blooded animals can live in cloſe veſſels. Experiment proves that frogs in vacuo die ſooner than newts; and I have found the ſame on keeping both immerſed in water. We may reaſon in a ſimilar manner on inſects, not to ſpeak of their different conſtitution, which may produce diſcrepancies.

It

It remains to inquire how the exhalations of refpiration are noxious to animals: and here in particular has Sig. Cigna difplayed his abilities. According to him, they induce death by irritation of the bronchiæ and lungs, forcing them to contract and corrugate, and thus prevent the admiffion of new air. Thus, in his opinion, animals in air infected by their breath die by fuffocation. He endeavours to prove that this is actually the caufe of death, from the various fymptoms obferved in the refpiration of confined animals. Refpiration becomes more frequent and fainter when the exhalations begin to collect, becaufe an equal quantity of air infpired containing more, it obliges the animal immediately to difcharge it. As they continue to increafe, refpiration alfo continues frequent, but becomes more laboured; and foon exhaufts the animal if confined in air where other animals have died. All this proves, in Cigna's opinion, that the exhalations injure the animal, by contracting the organs of refpiration by their irritating influence, and thus obftruct admiffion of the air.

In my numerous experiments, I have feen the fame fymptoms of injured refpiration. They are manifeft in warm blooded animals, and efpecially in birds. In cold blooded animals, they do not fo evidently appear, but are very perceptible on changing animals from open to confined air

air previoufly vitiated by exhalation. Sig. Cigna had experienced this. He confined a frog in a receiver where five or fix were already dead: the frog inftantly became agitated, and leaped violently againft the fides of the receiver; it was immediately affected by a frequent and laboured refpiration, which gradually became more painful, and foon ended in death. Therefore I fully coincide with the Turin profeffor, that refpiration is injured; but I cannot admit that fuch a contraction of the organs enfues as to kill the animals by fuffocation. Several frogs will live a long time in a capacious veffel. Their vivacity remains more than a day if the weather is not warm: after this they become fluggifh, and fwell exceffively. If males, the two veficles on the fides of the head increafe, and their inflation is fo great as to prevent the frogs from finking, and keeps them always on the furface of the water. Having remained fome time in this ftate, the frogs expire. On opening them, we difcover that the fwelling proceeds from inflation of the lungs, which cannot poffibly be more diftended. The fame inflation is found in thofe of toads and frogs dying in this manner; and they, too, are neceffitated to fwim. But the lungs are fo far from being rigid and contracted, that they are much dilated and confiderably charged with air. For this reafon, then, and for others of which I fhall shall afterwards speak, I cannot admit that animals die from obstructed respiration. Several vipers and frogs were confined in very small vessels where many animals were already dead, and an equal number put at the same time in vacuo. It is incredible how much sooner the former died; some did not survive a minute; but those in vacuo were alive at the end of several hours. It was not suffocation, therefore, or any obstacle to the air entering into the lungs, that occasioned death, otherwise the animals would have lived much longer, at least as long as those in vacuo.

Frogs were also forced to remain under water, and prevented from rising to respire at the surface. I have tied the origin of the lungs in such a manner that the air could not enter. Some I have deprived of the lungs, and confined along with other frogs in a small quantity of air very fœtid from the exhalations of respiration. The last expired in a few minutes, sometimes in one, even in less; while those prevented from respiring by immersion in water, having the lungs tied or cut out, have lived, as those in vacuo, for several hours. I have found the same with toads and water serpents. Since the death of all these species, by privation of respiration, has been without comparison later than the death of the same animals killed by the exhalations of respiration, we must conclude that these

these exhalations do not kill by suspending respiration, but that they are one of the poisons most fit to destroy life, acting as immediately as any other poison, and even almost instantaneously destructive when collected in a great quantity. This poison penetrating the body by means of respiration, when animals inspire the air, will cause that laboured breathing they suffer; for more than probable, it makes a violent and painful impression on the organs of respiration. At the same time, these are not the only vehicles for the poisonous vapours. Earth worms, leeches, and some other insects, which are not only without real lungs but also without stigmata or tracheæ, die in like manner with the rest in confined air (1). It is necessary to admit, that the exhalations act upon them, either by insinuating themselves through the pores of the skin, by the alimentary canal, or perhaps by both. The deleterious

(1) Vauqueline has made several experiments on the duration of certain animals' lives in confined air. A female locust lived thirty-six hours in eight inches of common air; and a snail lived forty-eight hours in twelve inches. He concludes, from various experiments, that insects and worms respire nearly in the same manner as warm blooded animals, and are capable of respiring vital air only. He found that worms died when all the vital air was consumed, *Experiences sur la respiration des Insectes et des Vers. Annales de Chimie, Tom.* 12.—T.

leterious quality of the exhalations is ſo terrible to all ſpecies of animals, that its effects are extended to thoſe that never felt the lively impreſſions of the air, from their conſtant abode at the bottom of waters. The ſnails and little ſnakes of ſtagnant waters ſhewed me this. They crawled over the bottom of an open veſſel of water, without betraying any ſign of uneaſineſs: but, when confined in a very ſmall veſſel, they became reſtleſs, aſcended the ſides of the veſſel, and, contrary to their uſual habits, left the water, and ſoon expired. Thus the influence of the exhalations acts under waters, which is indubitable from the fœtor communicated to thoſe in receivers, as well as to what ſurrounds them whenever it is copious.

But how do theſe peſtilential exhalations occaſion the death of animals, if they do not kill by depriving them of reſpiration? It is not by coagulating, diſſolving, or decompoſing the fluids. The blood preſerves its original fluidity immediately after the death of animals in this way, and flows in the ſerum; and its globules retain their ſize and figure. Beſides, if the exhalations coagulated the blood, or contributed to render it more fluid, they could not occaſion ſudden death; for animals deprived of blood die where they abound (1).

I have

(1) De Fenomeni della Circolazione.

I have fuſpected that this poiſon may be fatal by deſtroying the irritability of the muſcles, which is poſſible in two ways. The muſcular fibre may either become too relaxed or too rigid. No ſymptom of the muſcles being flaccid appeared, when the animals were taken from the confined air, but rather ſigns of rigidity. Such phenomena I obſerved in frogs: their hind legs and thighs were extended ſtraight out, as if they had been dried. On changing their poſition, or bending them, reſiſtance was felt; and their natural poſition was reſumed when at liberty. The muſcular ſubſtance ſeemed harder to the touch and under the knife. However I ſoon diſcovered that theſe changes in the fibre did not precede death, but followed it. If I took frogs dying, or juſt dead, from the veſſel, their limbs were not ſtretched, and the muſcles preſerved ſufficient pliability. On the contrary, extenſion and rigidity only took place when they remained ſome time in the veſſel. I have ſeen the ſame muſcular hardneſs and extenſion in the limbs of dead frogs continuing immerſed in water. Alteration in the muſcular ſyſtem by no means happens to all animals.

The following facts made me entirely renounce the idea of loſt irritability. The femoral muſcles of a frog diſplay great irritability when cut or pricked; they tremble and ſuddenly contract, not only where the point of the needle or edge of the instrument touches, but far beyond it; and the vibration continues some time, though the stimulus which occasioned it is removed. Having taken a frog on the point of death from a close vessel,—I will not say the muscles retained their irritability as in a state of health, or that the approach of death did not weaken it,—but I will affirm, that the vibration and contraction of the thighs, when cut and pricked, re-appeared, and even continued after the animal was entirely dead.

I abandoned the idea of lost muscular irritability: and, after deep reflection, it has appeared to me that the nervous system is the part on which the exhalations act. And here are the reasons which have suggested that opinion; they are submitted to the judgment of the philosophic reader. Convulsions commonly precede and attend the death of our animals: they are clearly manifest in frogs. Sometimes the whole body is convulsed, but particularly, and more violently, the limbs; and in these convulsions they die. As they resist death longer in winter, the convulsions also continue longer. If taken from the vessel before death, it appears that the seat of sensation has suffered. They are sluggish and motionless; nor do they change their place when stimulated, though agitation is evident; the convulsions re-

appear;

appear; their lethargy augments, even when put in the open air; and they generally die. Independent of being ſufficiently clear in itſelf, I obtained the moſt ſatisfactory proof that theſe convulſions proceed from injured nerves. I wounded the muſcles of a frog that had not been expoſed to confined air, and theſe motions which ſtimulating ordinarily awakens were excited; but they were never convulſive motions. On the other hand, by touching the origin of the crural nerves with the inſtrument, which was an extremely fine needle, the limbs ſuddenly became convulſed, and then exactly in the ſame manner as in confined air. When I pricked the ſpinal marrow or the brain, convulſions were univerſal over the whole body. I have never been able to ſee either general or partial convulſions by wounding any part of the body but the nerves. Thus, from the convulſive ſpaſms in animals confined in cloſe veſſels, I am induced to ſuſpect that the pernicious vapour acts on the nervous ſyſtem.

I had ſtill one doubt to ſolve. I could not reconcile the almoſt immediate death of frogs, expoſed to the powerful exhalations of reſpiration, with the preſervation of life a long time, though deprived of the brain. In my work on circulation, it has been ſhewn that frogs will live ſeveral days, though the brain is taken away. But the doubt diſappeared on ſeeing their immediate

 death,

death, when, inſtead of wounding the brain, the origin of the ſpinal marrow was wounded. If a pin was introduced where it united with the brain, the frog in a moment died convulſed. Animals, as tenacious of life as frogs, die equally ſuddenly, by injuring the ſpinal marrow. Therefore it is not very extraordinary, that death ſo immediate is occaſioned by quantities of the peſtilential exhalations inſinuating themſelves into animated bodies, and not affecting one part of the nerves or another, attack the whole ſyſtem, and momentaneouſly deprive it of ſenſation.

But what can we ſay of the death of thoſe animals in which there is found no veſtige of nerves, as the eels of vinegar and the hoſt of infuſion animalcula? As theſe animals actually periſh, the analogy of ſo many others dying from the ſame cauſe renders it plauſible that the exhalations deſtroy them by contact. Conſequently we muſt admit, that, acting on their organs, they produce an effect ſimilar to that which they operate on the nervous ſubſtance of other animals, notwithſtanding their organic ſtructure may be without nerves, at leaſt ſo far, that we cannot diſcover them by the microſcope. Therefore they cannot evite the fatal influence of the exhalations; nor do I ſee how they ſhould eſcape, while unable to reſiſt the effects of the electric vapour and odorous effluvia. I beſeech the reader to conſider this hypotheſis

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on the nervous ſyſtem only as a conjecture. I have not collected that aſſemblage of facts neceſſary to give it authenticity, nor have I had leiſure to enter into all the details, and make the moſt profound reſearches. I wiſh that others would undertake it; and I ſhall always entertain the ſame regard for thoſe who attain their purpoſe, whether they confirm or confute my conjecture; for I have no other view than the purſuit of truth.

In the firſt chapter, we have ſpoken of eggs and ſeeds which refuſed to germinate when confined in a ſmall portion of air. It is poſſible that this ſterility ariſes from the cauſes deſtructive of animals confined in cloſe veſſels. Compariſon will help to convince us. Butterflies, we have ſeen, do not come from chryſalids confined in ſmall veſſels. M. De Reaumur, I find, has had the ſame reſults, although the object of his experiments was different from mine. He hermetically ſealed glaſs tubes, four or five inches long, containing chryſalids, ſome had come from the cabbage caterpillar, and ſome from caterpillars that produce phalenæ: they conſtantly remained in their original ſtate, though confined above five months. They never unfolded; for, as he obſerved, they [*did not*] perſpire, and perſpiration is neceſſary for chryſalids to become butterflies. Theſe two facts he proved in a deciſive manner. When the chryſalids are in a very confined ſtate, as a tube of a few inches in capacity, the moiſture tranſpiring cannot diſſipate, on the contrary, it falls back on the chryſalids. Thus, in ſeveral days, they appear moiſt, and the humidity inſinuating itſelf into their bodies renders them diſeaſed. Therefore, the death of chryſalids enſues nearly from the ſame cauſe as that of animals in ſtagnant air. All this will properly apply to the eggs of inſects, and the ſeeds of plants. We know that eggs are not hatched until a certain degree of heat which promotes perſpiration. Confined in a ſmall veſſel, they reabſorb the exhalations that had before tranſpired from them, and theſe corrupt. The humidity covering the eggs, and ſometimes the ſides of the veſſel in conſiderable abundance, proves it. The ſame happens to vegetable ſeeds. I have often put them in cloſe veſſels, and, that they might germinate, in a little water. On taking them out, the part that had been expoſed to the air was viſibly covered with a humid pellicle.

For oppoſite reaſons, we ſee why eggs and ſeeds are developed in large cloſe veſſels; they are always in ſafety, for the vacuity is ſo great that the exhalations may diſperſe. For the ſame reaſon do butterflies come from chryſalids when the veſſels are capacious.

OBSER-

OBSERVATIONS AND EXPERIMENTS

ON

SOME SINGULAR ANIMALS WHICH MAY BE KILLED AND REVIVED.

SECTION I.

THE WHEEL ANIMAL.

In the hiftory of infufion animalcula, which I have treated fo much at large, it has been faid, when once they perifhed from a defect of water or humidity, they could never again be brought to life, though moifture was fupplied, and their immerfion continued long. Of this, I have had the moft convincing and repeated evidence in the experiments now to be narrated. But there are other animalcula which, notwithftanding they inhabit infufions, are fo much diftinguifhed and privileged by nature, as to enjoy the advantage of real refurrection after death. Such, among

 others,

others, are the *Wheel Animal*, the *Sloth*, the *Anguillæ* of tiles, and thoſe of blighted corn.

A microſcopic animalcule, inhabiting the ſand of tiles and ſewers, is called by naturaliſts the wheel animal. The abdomen is large, and ſituated towards the middle of the body: in the opinion of ſome, there is an heart. The poſterior part of the animal is provided with a minute trident, and the anterior divides into two trunks, bearing two moſt ſingular wheels at the ſummits. From theſe it has been named the wheel animal. One magnified is repreſented Plate 3. fig. 1. If the ſand we ſpeak of is put in water, and remains a certain time infuſed, the animalcule exhibits all its organs. If the water fails, the action of the wheels and heart ceaſes; the animal gradually loſes motion, and becomes lifeleſs: it contracts, grows very minute, and aſſumes the reſemblance of a dry emaciated ſkin, fig. 2. B. It is ſufficient to moiſten the ſand for its revival: then the body ſoon extends, the wheels and the trident appear, the heart is re-animated, motion is regenerated in the whole animal; it begins to ſwim, and exerciſes all the functions of life. That it has remained long dry in the ſand is of no importance. Leeuwenhoeck, who firſt had the good fortune to diſcover it, and from whoſe works I draw the chief part of what I relate, has ſeen wheel animals re-animated after being kept

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in dry sand almost two continued years. With this naturalist, we must observe, that the trunks and wheels are not always completely displayed when the animal revives, but are sometimes exhibited as in fig. 2. A. Such are nearly the three figures which Baker, after Leeuwenhoeck, has given in his Treatise, *The Microscope made easy*, where he contents himself with repeating what that excellent microscopist had written.

Though several naturalists have treated of the wheel animal, they seem to me to have done it but superficially, and chiefly to have proceeded on the accounts of Leeuwenhoeck. Thus I thought my trouble would not be misapplied in investigating this interesting subject, and illustrating it with additional facts; and I was particularly induced to it by the relation between it and the chief objects of those tracts. I have, therefore, composed a brief and methodical history of this wonderful insect from the materials which observation and experiment have afforded me; and it precedes the history of other animals enjoying the same privilege. When on the point of publishing the fruit of my labours, another of Baker's works written in English fell into my hands, where much is said of the wheel animal (1). I rapidly perused it; and at once perceiving that the

(1) Employment for the microscope, London 1764.

the author propofed to treat the matter *ex profeffo*, intended to fuppress in this work all that concerns the wheel animal, becaufe it would have been ufelefs to treat of a fubject already difcuffed by that learned obferver. I fhould certainly have done fo had I not obferved that Baker's obfervations were materially different from mine, becaufe his wheel animals were of another fpecies. I therefore determined to publifh my treatife, which was improved by it; its imperfections leffened; and new important matter added. This will appear from many parts of my obfervations and experiments which I proceed to relate, beginning with a fact which may be patent to every one. I examined the fand from a fewer about three hours after it had been put into water. It was not difficult to difcover the objects of my fearch. The firft drop, a mixture of fand and turbid matter, when prefented to the microfcope, exhibited three living beings, which I immediately recognized as three of Leeuwenhoeck's wheel animals. On the anterior part of the body was a horn; the body fwelled towards the middle; and the pofterior part was terminated by three points; but the anterior had neither trunks nor wheels, and the animals were nearly as in fig. 2. A. The body is tranfverfely annulated, and longitudinally radiated with fome parallel prominent rays, fig. 3. The indiftinctnefs of the rings and lines renders them

them difficult to be found; and one muſt be accuſtomed to obſervation, and have an acute eye, before he is able to ſee them. A ſmall longitudinal faſcia, covered with ſpecks, is obſcurely ſeen in the middle: and above it a circle, more viſible, formed as it were by two C's touching at the extremities. The origin of a little canal is ſeen at the upper part of the circle, A. B. fig. 3.

The animal being very flexile, it aſſumes ſome extraordinary ſhapes in its progreſſion. Sometimes extending, it becomes very ſlender, then it contracts into extreme corpulency. Sometimes the anterior part is contracted and concealed in the body, or the ſame happens to the poſterior part. Some of the body will be inflated, while the reſt is flaccid, or it will exhibit other motions as eaſy and ſingular to behold as they are difficult to deſcribe with preciſion; and all theſe remarkable figures ſucceſſively follow, though the animal remains ſtationary.

The wheel animal uſes the following method to tranſport itſelf from one place to another. It fixes the extremity of the tail to the plane which it intends to traverſe: then it extends the whole anterior part of the body. While in this ſtate, the animal detaches the tail, and, by contracting the poſterior part toward the anterior, it advances. The extremity of the tail is again fixed to the plane: the body is extended as before, the

tail

tail is detached; and, by contracting the anterior part, a new ſtep is made. The operation is repeated, and the animalcule paſſes ſo actively along as ſoon to traverſe the field of the microſcope.

This method of progreſſion, by means of contracting and extending the body, is common to many inſects, but eſpecially to apodal vermes, as is well known. A circumſtance peculiar to the wheel animal, is fixing itſelf by the point of the tail, which is eſſential to its regular progreſs, that without this precaution, it would have no motion except contortion or undulation. When the animal has found ſome points of ſupport, and is fixed by the tail, as to a centre, it frequently ſtops for ſome time, and ſtretches out the fore-part, as if examining around which way ſhould be taken; then, ſuddenly detaching itſelf, it advances in a given direction. In Leeuwenhoeck's opinion, the wheeler fixes itſelf by the three points terminating the tail. At firſt, I likewiſe thought them all neceſſary; however, with more attention, I perceived that the middle point only was uſed. To ſee it diſtinctly, the drop muſt be thin and tranſparent, and free of ſand; then we eaſily perceive, that ſo far from the lateral points fixing to the place of poſition, they do not even touch it, but are at a conſiderable diſtance, and the middle point is the only fixing one. When viewed with a powerful magnifier, this point seems composed of a number of infinitely fine similar points, which are almost imperceptible, fig. 3. D, therefore, correctly speaking, it is these points that fix the animal.

The three wheel animals, which I then observed for the first time, were not swimming, they crawled at the bottom of the drop. It was soon evident that this was their practice when the wheels were not in action; of which any one may be satisfied by putting a quantity of sand mixed with wheel animals into a watch-glass half full of water: he will immediately see, that those on the surface of the sand crawl over it, and do not commit themselves to the water. The same is the case with those buried in the sand, when by shaking they are brought to the surface.

The animals moved actively through the drop, searching as for food among the sand with the anterior parts, but they never went beyond the confines of the fluid; on approaching the circumference, they instantly returned.—Motion became languid as the drop began to evaporate, and the languor increased so much as to deprive them of the power of changing their place; though it all dried up, they continued to turn about and stretch themselves; such motions were most conspicuous in the head and tail, which proceeded from and re-entered the body, and were entirely concealed

concealed when the drop had evaporated. The appearance of the three wheel animals then changed, not only in the lofs of motion and all femblance of life, but, from a great diminution of fize, they became three minute corpufcula, fo diftorted that it was impoffible to recognife them for what they had originally been, fig. 4. A. B. C.

They were about an hour in this ftate of apparent death. I then put a drop of the fame water on them that had evaporated. The reader may well conceive my attention in obferving the refurrection, which was fuccefsful as I had anticipated. In a few minutes, the animals began to fwell, and a point appeared in one part, D, fig. 4. The pointed part moved by reciprocal extenfion and contraction; and the oppofite part, having alfo become pointed, began to move. Thefe points were the animal's head and tail, proceeding from the place where they had been retracted and concealed, on evaporation of the drop. The tranfverfe annuli, the longitudinal rays, the internal and external organs, all re-appeared. The three wheel animals foon refumed their original fize and figure; they traverfed the fand, and fhewed themfelves to be alive and moft vivacious.

Difcovering fome more wheel animals in the fand of fewers, I repeated thefe experiments, and found

found that they always revived, independent of the time they had remained dry. There is now before me a remarkable inftance.—Some fand is in my poffeffion on which I made experiments near four years ago, and it has been kept dry in a fmall glafs bottle: when moiftened, the wheel animals in it inftantly revive. This agrees with what has been quoted from Leeuwenhoeck. Baker obferved a fact little lefs worthy of notice. He wet the infide of a glafs, where wheel animals had been kept dry for fome months, and he faw them recover their original vivacity. It is of no confequence though they have revived oftener than once: the fame fand has been dried eleven times, and wet as often. I have uniformly feen them die as the water dried, and revive when the fand was moiftened.

However, thefe facts muft be underftood under fome limitation. Though the animals do revive repeatedly, and even after remaining long dry, it is certain that the number revived always decreafes in proportion to the time the fand continues longer dry, and the times it has been wet for their refurrection. It is true, I have feen their eleventh refurrection. The firft time, they were very numerous, but the numbers continually decreafed, and at laft became very fmall. It fhould be added, that, ftill wetting and drying the fand, none revived the fixteenth time. It is

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the ſame with reſpect to the ſand remaining dry. I took a portion from roofs, at the time numerouſly inhabited by the animals; and I have preſerved it dry in a box for three years, only moiſtening it every five or ſix months for my obſervations, but the reſurrections are always fewer; and now, at the end of the third year, it is no exaggeration to ſay, one third do not revive. I have not extended the experiment further. However, it is undoubted, if the wheel animals grow rare in the ſame proportion as they continue longer dry, a period will finally come when none will revive.

The time neceſſary to accompliſh reſurrection is unlimited. Some begin to live four minutes after moiſtening the ſand; life then extends to more; and in half an hour, all are reanimated (1). I am ignorant what can occaſion this difference in the time neceſſary for reſurrection. It may either be, becauſe ſome are in parts of the ſand better moiſtened than others,—or, although the whole ſhould be moiſtened at the ſame time, it may happen that all the wheel animals are not of the ſame texture. In that caſe, the more denſe or

(1) Baker's wheel animals began to exhibit life only in half an hour. It would appear, that he ſpeaks of thoſe that were longeſt of reviving. This might ariſe from his experiments being made on a ſpecies different from mine.

or consistent will more slowly receive the impression of the water, and be longer of reviving; or some may be diseased, and less fit for immediate resurrection. I have not perceived any very sensible difference of time between the resurrection of those that have been dry some hours, and the resurrection of those that have been dry several days, months, or even complete years.

As I knew the influence of heat in restoring the life of animals and vegetables, the sand was frequently moistened with warm water: and the animals revived sooner than when it was wet with water at the temperature of the atmosphere.

But there is one condition indispensible to the resurrection of wheel animals: it is absolutely necessary that there should be a certain quantity of sand; without it they will not revive. Let us enquire further into this. One day I had two wheel animals traversing a drop of water about to evaporate, which contained very little sand. Three quarters of an hour after evaporation, they were dry and motionless. I moistened them with water to revive them: but it was in vain, notwithstanding they were immersed in water many hours. Their members swelled to thrice the original size: but they continued motionless. This circumstance appeared to me the more extraordinary, as it was among the first times I had wet the sand, and of all the animals I had experimented on, these two were the only ones that did not revive. To afcertain whether the fact was merely accidental, I fpread a portion of the fand on a glafs flider, and waited until the numerous reanimated wheelers became dry in order to wet the fand anew. The fand was carelefsly fcattered on the glafs fo as to be a thin covering on fome parts and on others in very fmall quantity; here the animals did not revive, but all that were in thofe parts with abundance of fand, revived. A difference fo remarkable made me fufpect that, the ordinary dwelling of thefe animals being in fand, a certain portion muft be prefent to enable them to pafs from death to life.

However, to acknowledge the truth, though I did not at firft adopt this conjecture, I could not diveft my mind of it, as it feemed to be confirmed by facts; befides, on recollecting the experiments before made on wheel animals, they had certainly always been in fand. To corroborate or confute the fact, it was only neceffary to repeat the laft experiment; for, if thofe that revived had been mixed with fand at the moment of refurrection, or, on the contrary, if thofe without fand did not revive, it would be complete demonftration of what had been the caufe, namely, that the prefence of fand was effential to their refurrection. On repeating the experiment, it conftantly followed that the animalcula never recovered life unlefs in places where there was a quantity

quantity of ſand. One of my friends, an eminent philoſopher and an excellent microſcopical obſerver, has uniformly obtained the ſame reſult in his experiments (1). The Abbé Roffredi, a good obſerver, when incidentally ſpeaking of the wheel animal in the *Journal de Phyſique*, by the Abbé Rozier, mentions the like phenomenon.

To theſe, the following facts may be added. If the ſand containing wheel animals is ſpread out in ſuch a manner that a conſiderable quantity is in ſome places, much leſs in others, and very little in the reſt, and the whole then moiſtened; in the firſt caſe, where it is plentiful, the reviving animals will be numerous; where it is more rare, fewer will recover: And, in the laſt caſe, very few, or ſometimes none. If a dried portion of ſand, which was thick before, is ſcattered very thin and moiſtened, few wheelers will appear, though formerly numerous. Their reſurrection in a ſmall quantity is uniformly later than where ſand abounds; here it will be complete in four minutes: when there is little ſand, reſurrection will require nine or eleven minutes, and ſometimes more.

Have the animals not reviving from a defect of ſand, and reſembling globules floating in the water; have they loſt the reſurgent faculty, or do they reſume it on being ſupplied with their

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(1) Il Padre D. Carlo Giuſeppe Campi di Milano.

native ſand? I have often taken the thin ſurfaces of ſand, wherein the wheel animals did not revive, and put them in the bottom of a watch-glaſs with water: but of twenty dead, ſcarcely one revived. It therefore ſeemed that privation of ſand deprived them of the innate faculty of reſurrection (1).

How can the ſimple defect of ſand produce ſo important an effect? What connection, what phyſical relation is there between ſand and the reſurrection of wheel animals? May not the cauſe of this phenomenon be entirely different, and the ſand only ſupply the place of ſome very ſimple external condition? When the animals periſh where there is no ſand, their bodies are expoſed to the immediate influence of the air on evaporation of the water: but they are ſecured from it, at leaſt in a much greater degree, if they die covered with ſand. May we therefore affirm, that the lacerating influence of the air, irritating and injuring the corpuſcula while ſtill humid, moſt tender and delicate, renders them incapable of reviving from the alteration undergone? My conjecture is founded on a fact evincing there are animals

(1) I have ſeen the common water ſnail revive after deſiccation a conſiderable time. When ſome did not recover, I made ſeveral experiments with different kinds of ſand, but all have yet been ineffectual.—T.

animals whose structure is so fine and delicate, that, unable to bear the immediate impressions of the air, they always live under cover. Such are the *Miners*, a species of insects so named from inhabiting the interior of the leaves of trees, where they live almost always concealed and protected from the influence of the air. The conjecture would perhaps require an experiment which I had not an opportunity to make. We shall immediately see, that wheel animals revive in *vacuo*. One might put some of those surviving without sand into an exhausted receiver for the water to evaporate, and observe if they revived on being wet, which, according to my supposition, they should, because in that situation they could suffer nothing from the agitation of the air when the water evaporated (1).

We come to another inquiry more important than the preceding: I have hitherto supposed, that the wheel animals *perished* when the fluid dried. It is true they exhibit every appearance of death; the body is dry and disfigured; the

(1) Not only has the air a sensible effect on the tender bodies of animals, and they endeavour to escape from it, but there are several, even those destitute of vision, which cannot bear the influence of light, a substance infinitely more rare, and retreat from it as from a malignant agent. —T.

ufe and motion of the members are loft. But this muft be inveftigated more profoundly, fince it prefents the moft paradoxical truth to be found in the hiftory of any animal; and we cannot be too diffident and fufpicious of fuch facts. Let us, therefore, inquire, if it is not poffible that the animals, to all appearance dead, may preferve fome fpark of life. And here let us recur to the analogies between large and fmall animals. Cold, fo injurious to infects, renders thofe lethargic during winter which it does not deftroy: their torpor is fuch, that they feem dead to the fight and the touch: their limbs are ftiff and contracted; their wings depreffed, and their bodies emaciated. This we daily fee in hundreds of infects which we cafually find on the coldeft days of winter, in the earth, the clefts of trees, or the holes of walls. In this manner does cold operate on animals poffeffing the higheft rank in the animal fcale. In the midft of winter, we have found marmots fo lethargic, that the flame of a candle burning their limbs could not awaken them, or recall the fenfations of life (1). Terreftrial and amphibious animals kept long in water exhibit the fame appearances. Redi having immerfed flies in water an hour and a half, found them with all the appearance of death. Reaumur

(1) Buffon Hiftoire Naturelle.

mur made the ſame experiment on bees. Their vindictive diſpoſition, when rudely handled, is well known. He left a whole ſwarm, I know not how long, in water, and found them ſo completely deprived of ſenſation, that he handled them at pleaſure: he took them from the water, put them on a table, and examined whether or not there were ſeveral queens. The ſame may be ſaid of the apparent death of frogs and newts; after ſome hours immerſion in water, their bodies become flaccid and drooping, juſt as happens in death.

May not the apparent death of inſects and other animals be ſimilar to that of wheel animals among ſand? But they preſerve a real principle of ſenſation and life, which the concurrence of certain circumſtances is required to unfold and render capable of animating the whole ſyſtem. If the air becomes milder, motion and life return in the animals torpid from cold. If the bees and flies that have been immerſed in water are expoſed to the ſunſhine, they ſoon begin to move, expand their wings, and take flight. Frogs and newts recover their natural vivacity after being dry a little time. Why then cannot we ſay there is ſome latent ſpark of life in the wheel animals, which the aid of water is required to diſcover?

Considering these facts, and allowing their just value, we cannot deny that there is a resemblance between the state of dry wheel animals, and that of the animals we have named, with respect to appearance, perfect immobility, and complete inaction of their whole members. But there is seen a most remarkable and sensible difference, which must create a great distinction between them. In animals torpid from cold, whatever is the agent depriving them of sensation and motion, it only does so by deranging the necessary harmony between the fluids and solids; yet it does not derange them so far as to destroy what constitutes their fluidity or solidity. The same harmony subsists in the inmost parts of the body. I have repeatedly opened newts, frogs, toads, torpid by cold, and apparently dead; and I have found, that notwithstanding the blood did not circulate through the limbs, it continued to circulate in the large vessels, though with extreme languor. If a greater degree of cold has stiffened the solids, if it has coagulated the fluids, then it is certain that the animals perish. This, besides having been found by others in many insects, I have myself seen in the toads, frogs, and newts, of which I speak (1).

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(1) It is difficult to allow this its full extent. Blumenbach had a frozen frog which revived. Volta had several

I have alſo found a remnant of motion in the heart and blood of theſe animals half drowned, and doubt not that it continues in bees and flies. But if they remain long in water, all internal motion is deſtroyed, and every hope of recovery is gone. It is therefore indubitable, that in theſe animals returning to life, the quality which conſtitutes the exiſtence of the fluids, or ſolids, is not taken away, nor is the harmony that reigns between them totally deſtroyed. How very different is it with regard to wheel animals. When moſt vivaciouſly traverſing the fluid, their body reſembles a thick jelly; the touch of the point of a needle is ruin and deſtruction. When dry, the ſolids are contracted and diſtorted, the whole body of the animal is reduced to a hard ſhapeleſs atom of matter; pierced by a needle, it flies in pieces like a grain of ſalt. How is it poſſible that this atom, whoſe ſolids preſerve no veſtige of their former humidity and pliancy, and where the fluids exiſt no more, how I ſay, can we ſuppoſe, that in this dry and disfigured atom, a principle of life remains? Does animation exiſt in a frog, a toad, or a newt, when as dry

veral frozen for months, which ſtill exhibited ſigns of Galvaniſm; and the author afterwards kept a number of the ſame animals two years torpid in ſnow. They were dry, ſhrivelled, and friable; but revived on the application of heat.—T.

dry and rigid as wheel animals are among the sand? May we conclude, and conclude with reason, that in them and other resurgent animals, life is entirely gone, not only because the reciprocal actions of the fluids and the solids is destroyed, but because the fluids are entirely evaporated, and because dryness and rigidity has changed the natural state of the solids? If we saw a stiff and contracted frog, toad, or newt, gradually revive when put in water, as we should call it a real and absolute resurrection, so should we call that which happens to resurgent wheel animals a real and absolute resurrection also.

But it is time to resume the history of these wonderful animalcula: We have already described their figure and properties, but we have not yet examined their organs separately, which is essential before becoming well acquainted with both. By these I mean the heart, the two trunks, and the wheels, acting at the vortices. I could not properly do this before, because it would have obliged me to deviate from my intended plan, which led to the relation of some facts following the order of time when they had occurred. It was in the prosecution of my observations, that the animal exhibited all the three organs. While animated, it frequently happens that the whole are not shewn at all during the time of animation, or displayed very slowly. This

This is what I have obſerved in my wheel animals, and what ſome of my friends have obſerved along with me. I did not ſee the three organs till after twenty-one days examination. Let the reader figure a ſnail proceeding from its ſhell; it extends itſelf, and puts out the head and horns; then retiring within its habitation, it is contracted and conceals them within the body. In this manner he will ſenſibly conceive the motions of the two trunks and wheels of our animalcula. Thoſe then examined, and afterwards ſeen, did not always diſplay both the trunks and wheels at the ſame moment, but, like ſnails, ſometimes concealed the one and ſometimes the other, which happened whenever they contracted themſelves; and when remaining long extended, the trunks and wheels were kept out a long time alſo. The wheels receive this appellation in a very improper ſenſe, and by means of a ſort of latitude or accommodation.—In the tract on infuſion animalcula, I have treated at length of the minute, long, and ſlender fibrils proceeding from the edge of the mouth of many of theſe beings: I have ſaid they were in continual vibration; that they produced a certain vortex in the infuſions, which carried the corpuſcula feeding the animalcule to its mouth. The wheels of wheel animals are only two circular lines of ſimilar fibrillæ conſtantly in motion: they produce the ſame effect with the

the vibrating points or fibres of infusion animalcula, forming two great vortices which convey the animal's food to it. A wheeler is exhibited, with the trunks extended, and the fibrils whose motion resembles that of two wheels, and forms two vortices, fig. 5, plate 3.

When I say, the wheels of my animalcula are such only in appearance, I do not mean that this will extend as a general rule to all. The optical illusion indeed has been corrected by some naturalists, particularly M. Trembley and Bonnet; but it is certain, that the opinion of others is different. Leeuwenhoeck, that deep and acute observer of the most minute objects, actually calls them wheels, which revolve like those constructed by mechanics. Baker, who is not inferior to him in accuracy of observation, and has studied the wheels most attentively to discover whether they are truly such or only vibrating fibrillæ, is more inclined to believe them wheels. What these two able naturalists have observed may very well coincide with my remarks, for their wheel animals have been specifically different from mine. A singular aperture for a mouth situated between the wheels, a sort of ring beneath it, a number of serpentine vessels in the head, the peristaltic motion of the intestines, the irregular agitation of a transparent fluid in every part of the body, a particular undulation of that fluid

fluid in the inteſtines and ſkin, were ſeen by the Engliſh philoſopher in his wheel animals; and although he has accurately deſcribed them, I have been able to find none ſimilar in mine. There is no doubt that I might have ſeen all theſe organs, both becauſe I uſed Cuff's microſcope, as Baker had done, and alſo ſome which were much ſuperior. With this different organization, it is not wonderful that his wheel animals exhibited another organ which I have not found in mine; that is, a pair of wheels proceeding from the two trunks, whoſe revolution produces the ſame effect as vibration of the fibrilli, and forms a moſt rapid current, which carries particles to the animal's mouth. It muſt be remarked, with Baker, that this apparent rotation is not always executed with equal velocity, nor in one direction. Sometimes it is very quick, ſometimes very ſlow; and theſe alternatives are either inſtantaneouſly or gradually effected. The animal, at one time, turns to the right or the left; and, after moving long at one ſide, it often interrupts the vortex and begins it in a part diametrically oppoſite.

Let us leave this brief digreſſion, and return to our animals. They no longer crawl at the bottom when the fibrilli appear, but ſwim through the whole fluid with the greateſt velocity. Examining them while in the act of ſwimming, I have

have often endeavoured to discover whether they swim by undulation of the body, or by vibrations of the fibrilli, which, besides producing the vortices, may raise and conduct the wheel animal by their action against the water. But it has not been in my power to elucidate the fact satisfactorily; however, I should incline to think they swim by means of the vibrating fibrilli, since they generally cease when these are drawn within the body (1).

I have already spoke of a little circle situated towards the head, which appears like the junction of two C's by the extremities. This part is in constant motion by alternate contraction and dilation, while the animal forms the vortex, and the fibrils are extended. Both Leeuwenhoeck and Baker have observed it, and thought it the wheel animal's heart. Are we sure it is this organ? The situation, figure, contraction, and dilation, concur, according to the English naturalist, to support the opinion. But if it is a heart, it is a

(1) Some animalcula certainly may swim in this way; but the wheels of this animal in particular, bear so small a proportion to the size of the body, that it is difficult to conceive their power to convey it along: besides, its specific gravity is greater than that of water. I should imagine it more probable that the wheel animal swims by undulation.—T.

a voluntary muscle, which beats at the will of the animal; that is, when it protrudes the fibrilli and forms the vortex: and this *spontaneity*, if I may use the expression, has, before me, been observed by others. Are there animals whose heart beats by intervals, so that pulsation may cease when the animal chuses? Wheel animals sometimes remain several weeks alive in water without making the vortex, consequently without moving the heart. Is it possible that any animal can exist so long without pulsation of the heart, the animating spring of the whole machine? These are two paradoxes which may be no less true than others more wonderful, such as the resurrection of the wheel animal. Though this particle may be thought a real heart, either from performing functions similar, from its situation in the region of the breast, from contraction and dilation like another heart, still these are not convincing reasons, because it may be an organ destined for very different purposes. I ought to say, as I think, that it is more natural to believe it to be an organ serving for the aliments; and that the contraction and dilation is for receiving the food and transmitting it to the stomach. Such an hypothesis will easily explain why it is in motion only while the vortex is formed: it is because aliment is then drawn to the mouth and transmitted to the body. If the part remains long motionless,

motionless, it is becaufe no food is taken, and this commonly happens when the animals, being in an unfuitable fituation, languifh and die; which is fometimes the cafe with thofe revived in fand kept in clofe veffels. Then I have feen, that although the fand fwarmed with wheel animals during the firft days, the number decreafed, and thus continued to fuch a degree, that in twelve or fifteen days the whole were dead. They appeared lifelefs and disfigured at the bottom of the veffels, and many were even reduced to nothing. During this period of difeafe, the vortex was feldom made; but it is almoft always formed when we find them in fewers, in pits or holes of rain water (1).

It is not an imaginary idea that this particle is an organ formed for the receptacle of the aliment, and to tranfmit it to the ftomach. It is founded, 1. On obferving in my wheel animals a kind of little canal united to it, fig. 3. B. 5. E. which, rifing towards the head, greatly refembles an œfophagus. 2. This part is furely appropriated for that purpofe in other aquatic animals, bearing great

(1) 'The heart of the wheel animal, which both Scopoli and Spallanzani affirm is at reft when its filaments 'do not move, I have fometimes feen in motion. It is 'rather a mufcle for deglutition.' Muller, Anim. Infuf. p. 297.—T.

great relation to wheel animals, which may afford a ſtrong corroboration of the fact. Such is an animal often found on the tremella, ſhorter and a little thicker than the wheel animal. The poſterior part is provided with two diverging filaments, with which it fixes itſelf to any ſubſtance. At the anterior part are long ſlender fibrils, occaſioning a vortex in the water when the animal puts them in motion, fig. 6. There is no motion when the fibrils are at reſt. While moving, and during the continuance of the vortex, a particle, A, ſimilar in figure to that we ſpeak of in the wheel animal, is ſeen almoſt in the centre of the animal: it alternately contracts and dilates, but motion ceaſes with ceſſation of the vortex. This difference only is to be remarked; the particle of the wheel animal is formed by two ſemicircular cavities, whereas that in the tremella animal reſembles a bladder or folliculus. The particle and canal, towards the region of the head, are connected, as in the wheel animal, by a ſhort duct, B, terminating at the mouth of the animal, and at the oppoſite extremity it enters another folliculus, C, which not only moves with alternate contraction and dilation, but undulates like a wave at reſt. This folliculus is preciſely the receptacle of the aliments. It is always full of a yellowiſh green matter, which, from time to time, proceeds from the

the posterior part by means of the undulating or periſtaltic motion. But we not only obſerve the food diſcharged from the body, we ſee it enter; that is, fragments of the tremella are ſeen conveyed by the vortex to the animal's mouth, infuſion animalcula of various ſizes, and particles of other ſubſtances. Some of the moſt minute enter the origin of the œſophagus, traverſe the moving particle, and arrive at the paſſage for the receptacle of the aliment (1).

The ſame thing is obſerved in another animal of the tremella, which is mentioned in the tract on infuſion animalcula, pl. 1, fig. 10, R. The moving particle of this animalcule, for it has one as well as the wheel animal, contracts and dilates, while the aliment, collected by the vortex, paſſes from the œſophagus to the ſtomach. Thus we ſee a particle in theſe animals, which, in ſituation, figure, and motion, reſembles a heart, although it is not one, but an organ deſtined for the uſe of the aliment. For theſe reaſons do I aſcribe the ſame uſe to that of the wheel animal.

If I am right, the wheel animal has no heart: we ſee no other part, no other organ, that

(1) This ſeems to be Muller's *Vorticella Furcata*, p. 299. There is no figure of it among the plates, for which a reaſon is given in a note. I do not remember to have met with it.—T.

that can merit the name. If we may judge of this by the ſenſes, I ſay, it has none more than the two tremella animals, many infuſion animalcula, the prodigious number and variety of polypi viſible by the microſcope and to the naked eye, to omit many animals which it would be tedious to enumerate. I have never ſeen the appearance of circulation in wheel animals, infuſion animalcula, in thoſe of the tremella, or in polypi. Although Baker has obſerved the irregular agitation of a fluid in wheel animals, he ingenuouſly avows that he has never perceived any trace of real circulation; yet all feed, increaſe, and multiply, as thoſe animals which have a heart and circulation. Neither are they eſſential to the vital functions of many; for theſe, it is enough that there is a juſt equilibrium, a correſponding harmony between the fluids and the ſolids. The idea we form of a heart and circulation are particular notions derived from a definite number of animals, which demonſtrate the limits of our knowledge and underſtanding, and would ill apply if we meant to adopt them to the immenſity of models framed by nature.

The wheel animals inhabiting the roofs of houſes, towers, and other buildings expoſed to the inclemency of the weather, ſhould be of a conſtitution calculated to ſupport the ſevereſt effects of heat and cold. I put them to the teſt.

From a fewer in a fouth expofure, I took wheel animals' fand, which had been expofed to the heat of the fun twenty-nine days in the middle of fummer. The thermometer ftood at 129°, 133°, 138°, in the funfhine; but the animals were not injured, for I had a great number very vivacious when the fand was wet.

A little of this fand was expofed the whole fummer, in very thin glafs tubes, on the outfide of a fouth window, where the reflection of a neighbouring wall excited extreme heat. During fome of the hotteft days, the thermometer rofe to 142°; but this did not injure the wheel animals. On wetting the fand, they appeared with the fame livelinefs and vigour, and in the fame abundance, as in other fand from the fame place expofed in a north window, and feldom or never experiencing the folar rays. Therefore it is evident, that the exceffive heat of fummer is not fo prejudicial to wheel animals as to deftroy the refurgent faculty.

But is it the fame when revived? Is this degree of heat equally fupportable? I have alfo expofed the tubes with fand and water, containing a great number of living wheel animals, in the fame warm fituation. The confequence was very different. In half an hour, the heat of the fun at 135° killed them. Thus, it is otherwife with wheel animals, dry and deprived of life, and when they are animated and in motion. I afterwards

wards saw that resuscitated wheel animals died at a much more gentle degree of heat, when exposed in the sunshine at 113°.

The heat of the fire has the same effect as that of the solar rays. Although the revived animals perish at 111° and 113°, if dry, they do not lose the power of resurrection at 144°. I could extend my experiments further with common fire than the sun. The heat was raised above 144°, to see whether dry wheel animals would revive; for it was probable there were limits here, and these I found at 153°. Sand exposed to this heat, presented few wheel animals; and exposed to 158°, none appeared. But there is one circumstance necessary to be remarked. The experiments were made dry, that is, keeping the sand two or three minutes exposed to the heat. The consequences were very different on using wet sand, immersing it two or three minutes in water warmed to that degree: then the animals did not revive after 131°.

It is not difficult to explain why the destruction of reanimated wheel animals is easier than when in their state of desiccation. The former are a kind of jelly, consequently most delicate. Their minute filaments are easily broken and destroyed by the penetrating power of heat, which cannot operate with such facility when they are dry: then the parts are concentrated within themſelves, compreſſed and indurated. Beſides, in this ſtate, their globular figure preſents leſs ſurface to the action of the fire. The heat acts alone on dry wheel animals; when alive, it acts in conjunction with the water, which powerfully concurs in lacerating them and deſtroying their organization, from its particles being ſubtilized by the heat and rendered more active and penetrating. Thus it is that dry wheel animals can reſiſt the heat of warm water leſs than that of the fire itſelf.

Having ſeen the effect of heat on wheel animals, it was neceſſary to ſee the effect of cold. With this intent, I took the ſand of ſewers and from the hollows of the eaves and tiles, where they are found during the moſt intenſe cold of winter, when roofs are covered with ſnow and ice. The ſand, moiſtened with water, became ſo firm and connected by the cold that it was as hard as a ſtone; but the wheel animals were not injured. After melting this mixture of ice and ſand, a great many revived; however, their reſurrection appeared leſs immediate.

The greateſt cold of winter was 16°. I therefore determined to expoſe the wheelers found on roofs to a degree more intenſe: and taking ſome portions of frozen ſand from the bottom of a ſewer, I put them in a glaſs veſſel which was placed three hours in cold 11° below 0, obtained ed by means of the mixture I have often mentioned. The revival of the animals, after the ice melted, proved that they had suffered no injury.

I next sought for the result of the experiment inversed; that is, what would happen on transmitting wheel animals, from the degree of heat at which they were animated, to various degrees of cold always more intense. One morning, some were taken in a watch-glass to a north window, where the thermometer stood at 25°, and I attentively observed what passed. When the water became so cold that the hand could scarcely be kept in it, the wheel animals interrupted the vortex and fell to the bottom, crawling languidly over the sand. The water soon froze; then they moved with difficulty, and soon ceased. When more frozen, they contracted within themselves, forming into globules which were clearly seen from the transparence of the ice. Thus they passed the whole day and following night, which was very cold. Next day, I removed them to a warm situation, to see whether those in the ice, under the figure of globules, would recover when it melted. They did so, even when remaining longer in the ice, and when the natural cold was increased by means of fictitious to 11° under 0.

Reasoning from the experiments by cold as from those by heat, it might be inferred, that revived wheel animals should not support the same degree as those which are dead; if, on the other hand, these last facts did not evince that when the cold begins to act powerfully on those revived, they pass from life to death, as appears by the cessation of motion, their contraction and disfiguration, so that they become exactly as when evaporation of the water leaves them dry in the sand. But I do not know that the action of extreme cold will deprive living wheel animals of life. It is certain that it could not destroy animalcula equally delicate, such as some infusion animalcula and the eels of vinegar. Yet, what is more surprising, the animalcula of infusions and the eels of vinegar perish with less heat than wheel animals: they cannot support more than 111°. At that degree several species of beetles, chrysalids and caterpillars, perish, as I before observed, though they can support 34° below freezing. This shews that many animals in similar situations can support cold better than heat.

From the facts hitherto related, we collect that there are two principal causes destructive of the resurgent quality in wheel animals, the want of sand and of heat. Are there others also producing the same effect? I could discover it only by conjecture, and using the different methods prejudicial to the production and life of other animals, especially of those bearing the greatest analogy

logy to wheel animals, as the animalcula of infusions. It has been proved that these are produced in vacuo. This seems an effectual method for preventing the resurrection of wheel animals, though we cannot deny that their resurrection is facilitated by the influence of the air. The principal results of repeated experiments, are, 1. Wheel animals revive sooner and in greater number in the open air than in vacuo. 2. Those that do not revive in vacuo, recover when put in the open air.

However much air may promote the resurrection of wheel animals, it is absolutely necessary for the preservation of their lives. When they revive in vacuo, or are put into an exhausted receiver, they die in a few days.

If wheel animals did revive in vacuo, though not so successfully as in the open air, it was very reasonable to suppose they would also revive in confined air, though it is one method of preventing the developement and of occasioning the destruction of other animals confined in very small vessels. I sealed up some with wet sand in vessels: they always revived very soon, and in abundance: they have even lived long, though, from the extreme smallness of the vessels, there could be very little air.

Wheel animals suffer from many fluids what they do not suffer from privation of air, or from air

air confined. The liquids injurious and advantageous to them are the following: Those innoxious, I understand to be such as either revive them or preserve them alive when recovered; and of this nature are, pit, river, ice, snow, and rain-water, distilled water, that of ditches, marshes, and pools, the fœtid water of mud and dunghills. With respect to the fluids pernicious, they are either those impregnated with pepper, common salt, sal-gem, sugar, vitriol; those in which are expressed the juice of onions, garlic, urine, ink, wine, verjuice, oil of olives, or nuts, brandy, vinegar, and the like. I never saw wheel animals revive when their sand was put into any of these fluids, and all the revived animals put into them perished. Some strong and penetrating odours have been equally fatal, such as that of camphor. All the living ones long exposed to its effluvia die, and these exposed dry do not recover. The oil of turpentine produces only the first effect. But if the odour becomes more active, as by melting or burning the turpentine, the fumes prevent the revivifcence of the animals. Both effects are produced by the fumes of burning sulphur and camphor; only revived wheelers are destroyed by those of leaf tobacco.

Reflecting on the experiments by means of heat, liquids, and odours, I have sometimes doubted whether these three agents had deprived the

the animals of the refurgent property for ever, or if there was any reafon to hope that it might be recovered. In a being like the wheel animal, this hope did not feem chimerical, nor would it have been wonderful to fee it recover the faculty which it naturally poffeffes. I have preferved fand that had been expofed to heat, and from time to time wet it with pure water, and often obferved it. The fame has been done with fand expofed to liquids and odours, keeping it in the air and weting it with frefh water, that the noxious qualities which injured the wheel animals might be deftroyed. But the numerous dead bodies have never been re-animated by thefe methods.

The wheel animals, which fuggefted the opinion I have laid down, were for the greater part found in fewers and the hollows of tiles, among a fubftance which, for brevity, has been called fand, though, to fpeak more properly, it is a mixture of earth, fand, and the fragments of tiles. This fand, for I fhall continue to call it fo, is the abode of wheel animals, but in fome kinds they are much more numerous than in others. It is fingular, that if the fand is reddifh, it is almoft a certain indication, according to Baker, of their prefence. They are always inanimate when the fand is dry. By one accuftomed to obferve wheel animals in the dry ftate from evaporation of the water, they are eafily recognized

recognized when the sand is presented on a slider to the microscope. Then they are in the figure of minute dry globules of a reddish yellow colour, which, by humectation, expand into so many animated wheelers.

Besides roofs and tiles, particular waters contain them. Both Baker and myself have often seen them in ditches; and I have taken many from pools, marshes, and even holes of standing water.

The wheel animals of the earth are, in my opinion, the origin of those of roofs: and this seems a necessary conclusion; at least we cannot say those of one roof come to another, which supposes a particular case: but to speak generally, and consider the origin of the matter, we must derive their origin elsewhere, consequently recur to the waters. The manner in which they pass from the earth to roofs, may be easily conceived; in their dry state, the wind may transport them through the air, whenever their native element is entirely or partially dried up.

In my studies on these curious beings, I have always reflected on a most important problem, to inquire into their mode of propagation. With this view, I have isolated them in watch-glasses like infusion animalcula, putting one in each; but I could never observe them propagate either by shoots or division: though both ways are common

common among aquatic animals. Neither was it by a foetus, but I had reason to think it was by means of eggs. When the animals had been some days revived, an ovular substance was seen in the body of the largest, Pl. 3. fig. 5. N: and when I happened to find them dead, they always had this ovular substance. But it had in general passed from their bodies into the glass without the means being known; at the same time with an important singularity: when entire, the isolated animal swam alone in the fluid; but when the substance was broken, another wheel animal much smaller swam along with it. This made me suspect, that the new inhabitant had come from the ovular substance, which, as other eggs, was broken for exclusion of the animalcule. One might suspect, that it also was carried thither by the air; however, to ascertain the fact, it was necessary to see the wheeler issue from the ovular substance, which, notwithstanding all my care and attention, I could never accomplish.

Baker's observations agree with mine, though he has not been more fortunate. He thinks wheel animals are oviparous, because he has often observed a considerable number of gelatinous eggs of proportional size in the water along with them. He has also observed in a species of wheel animals, a little larger than the most common kind, an elliptic body, the figure of which very

very much resembles the substance I have describ-ed: but he never had the satisfaction to see one excluded: nor has he seen a wheel animal come from the gelatinous eggs, though he kept them three years.

The learned Abbate Roffredi has enjoyed the good fortune denied to Baker and me, and put the finishing stroke to our observations. Incidentally speaking of the wheel animal in Rozier's *Journal*, he says, in express terms, he has seen it delivered of an egg, and an animal proceed from the egg. 'Leeuwenhoeck is mistaken 'in thinking the wheel animal viviparous; and 'what he supposed excrements in the intestines is 'really an egg, which I have seen it produce, and 'often continued to observe until it was hatched.' If Roffredi's observation is correct, which, indeed, cannot be doubted, a fact in the history of the wheel animal is elucidated which should deeply interest the naturalist.

The solution of this problem, added to my observations, discovers another important truth, namely, that wheel animals undergo no metamorphosis. I have collected many minute ones produced in a watch-glass. They always continued growing; but when killed and revived their increment was slow; and quick, on the contrary, when they were kept continually wet. When full grown, each generally laid an egg, which

which produced another wheel animal. Thus I know, from the time of exclusion until maturity, they undergo no change. We cannot call it a metamorphosis when they are produced, for then they have attained their greatest perfection. The insects that metamorphose never propagate their species until they become winged animals, that is, until they have acquired the last degree of perfection to which metamorphosis will bring them.

Finally, my observations have shown me, that wheel animals are hermaphrodites in the most rigorous sense. I have obtained the fifth generation from many eggs which were isolated in watch-glasses to remove all suspicion of copulation.

SECTION II.

THE SLOTH, ANGUILLÆ OF TILES, AND THOSE OF BLIGHTED CORN.

The sand of tiles, the mud of ditches and marshes, which pass in the vulgar eye for the vilest of matter, are sources of wonder to the philosophic observer, ferver, from the rare and fingular beings they contain. To the mud of ditches and marfhes we owe the clufter, armed, bulb, funnel, and knotted polypus. It is there we find the frefh water worm; the boat worm, and the dart millepede, animals that have confounded the human mind, and created a new philofophy. When the fand of tiles is not the abode of wheel animals, it is not then the lefs famous or remarkable. An animal which revives after death, and which, within certain limits, revives as often as we pleafe, is a phenomenon as incredible as it feems improbable and paradoxical. It confounds the moft received ideas of animality; it creates new ideas, and becomes an object no lefs interefting to the refearches of the naturalift than the fpeculation of the profound metaphyfician. But the celebrity of this fand will increafe, by learning that it contains other animals, which, like the wheeler, poffefs the property of refurrection: fo that we may almoft fay, all the animals living in fand are immortal. There I have difcovered two new fpecies of animals, which I proceed to defcribe. I lament that their rarenefs has prevented me from extending my obfervations as far as I could have wifhed, or rather as far as the importance of the fubject would have required.

On wetting wheel animals' fand, I feveral times obferved a yellowifh animal three or four times larger

larger than a wheeler with ſix legs; but I paid no particular attention to it, ſuppoſing that it was ſome little terreſtrial inſect that had caſually fallen into the watch-glaſs where the ſand was kept. My reaſon for thinking ſo, was from always having ſeen it move obliquely and very ſlowly at the bottom of the water, as if unable to walk, and often ſupine, making great exertions to recover its natural poſition, but they were in general fruitleſs, as happens to many aerial and terreſtrial inſects caſually falling into water. At the ſame time, with more continued and careful obſervation, I recognized it as an animal really aquatic, and perceived that its awkward and laborious mode of progreſſion was from the ſmoothneſs of the glaſs ſlider on which it had been put for examination, and, when placed on ſand, that it had a regular progreſſive motion, ſlow indeed, and, compared with the wheel animals' motion, like the crawling of a tortoiſe. Thus to deſign it by ſome deſcriptive name, I called it the *Sloth*.

The whole body is granulated: the anterior part obtuſe: and the poſterior terminated by four hooked filaments, which ſerve for attaching it to any particular place. The limbs have ſmall ſhining claws, or nails, which, as far as one can judge, are of a corneous ſubſtance, the points turned towards the body, as we ſee in the re-curved claws of ſeveral inſects. The corpulence of the ſloth, rendering it opaque, prevents us from ſeeing the internal organization. But we can perceive a ſmall elliptical ſpot in the middle of the body, which I ſuſpect to be the reſervoir of the aliments. In the anterior part is alſo diſtinguiſhed an internal lucid ſpot, ſmaller, narrower, and longer than the other, which I have ſometimes ſuppoſed the œſophagus. The figure of the whole is clumſy, and very much reſembles the teſticle of a cock. The ſloth is repreſented ſupine, fig. 7. pl. 3. the profile is ſeen fig. 8.

This animalcule forms no vortex in the water, which is not ſurpriſing, as it has neither the wheels nor fibrillæ of the animals that perform this operation. It appears that the wheel animal cannot advance a ſtep without fixing the trident to ſome adjacent ſubſtance; it is otherwiſe with the ſloth, for it often makes no uſe of its hooked filaments. It never ſwims; it is ſpecifically heavier than the water; thence it always turns round on the ſurface of the ſand, or amongſt it.

The phenomena of its death, from the want of water, and of reſurrection when water is ſupplied, are preciſely the ſame with thoſe of the wheel animal. Motion gradually ceaſes: the limbs are contracted and drawn entirely within the body, which diminiſhes very much, is completely dried, and aſſumes a globular figure, pl. 4. fig.

1. The reverſe ſucceeds when the ſloth is revived by ſupplying water. As the wheel animal can only revive a certain number of times, ſo it is with the ſloth. And, although ſand is neceſſary for its reſurrection, it does not appear ſo eſſential as for that of the wheel animal.

The degrees of heat, fatal to revived or dead wheel animals, are alſo fatal to ſloths; and the ſame may be ſaid of odours and liquors. Cold, however intenſe, does them no harm, and in this they likewiſe coincide with wheel animals.

Sloths are infinitely more rare than wheel animals: for five and twenty of theſe, four or five ſloths are hardly found. All are of the ſame figure, but not equal in ſize. I have iſolated many in watch-glaſſes, ſometimes with ſand and ſometimes with pure water, intending to diſcover their mode of propagation, but, inſtead of multiplying, all periſhed: ſome ſooner, ſome later, none ever attaining the ſixth day (1).

 The

(1) This animalcule is uncommonly rare. I have never ſeen a deſcription of it by any other naturaliſt: and it only occurred to me in two infuſions. Muller's *Cercaria Hirta*, which he found but twice, ſeems to have ſome reſemblance, but he does not ſay enough of the properties to aſcertain it, nor does he remark any feet. I had mine from June until the following February. They were very

The third species of resurgent animals found in sand consists of certain minute eels, very like the anguillæ

very minute, not one tenth of the size of those mentioned in the text, which, added to their rarity, was an absolute bar to experiment. They had no terminating projections or hooks, nor were the different fascia visible. However, I sometimes thought two hooks of very few were perceptible, but I could never be certain. The animal was perfectly opaque, which prevented any observation of the interior part. There was no sand in the infusion, which had been made of dried twigs eight or ten months before, and the sloth's natural abode was among the particles of matter, especially on the twigs. How it propagates I am ignorant: it did not multiply much.

I have discovered another animal which most probably belongs to this singular class. It was in an infusion the same as the former, and there I had it from the beginning of May until the end of August. It then disappeared, and I saw one in the subsequent February. In one other infusion, I found two or three nearly the same. It bears the greatest resemblance to a most minute caterpillar, both in appearance and motion. It moves little, and then with great awkwardness and languor. The largest might be about one-third of a line in length, and perceptible by the naked eye, which could only distinguish a long white speck, but few are of this size. Instead of six legs and four hooks, there are eight legs, the two last exactly like those of a caterpillar. With the second highest magnifier of my microscope, I could observe each of the six legs anguillæ of vinegar. This ſpecies is much rarer, nor is it to be found on every roof. The head and

legs terminated by two long claws a little curved; whether the two hind ones were terminated by three, four, or five, could never be aſcertained; circumſtances were very ſeldom ſo favourable as to admit of the others being ſeen. In ſome, I thought the eye might be diſtinguiſhed, and an aperture for receiving aliment. There is a great difference of ſize. Moſt of them are of an opaque duſky brown, which ſeems their natural colour. Some are perfectly tranſparent, which I ſuppoſe are dead, for no motion has ever been evident, although the fact is not abſolutely certain.

I know very little more of the propagation of this animal than that of the former, except from conjecture. One night in May, I was ſurpriſed to ſee three large round ſubſtances in the body of a ſloth: it made no impreſſion on me at the time; but returning to obſervation, ſeveral nights afterwards, I ſaw the body of another, of the largeſt ſize, completely filled from end to end of the whole with ten large eggs. I do not think there was room for one more. Every moment I expected to ſee an egg proceed from the body. The obſervation was continued for hours, and during the next day. The ſame number remained: the animal was then obſcured by ſome particles of matter, and all endeavours to recover it ineffectual. Nor has my ſucceſs been greater with the few others appearing with eggs; for, notwithſtanding all poſſible care and attention, I never could ſee one produced. The ſloths containing them were all perfectly tranſparent and motionleſs. The and adjoining part of the body are very transparent, and of a shining silver colour: the tail is the same, but the intermediate part is darkish and all granulated. The greater part of the tail is bent, and terminates in a very sharp point. The head, on the contrary, is obtuse; and a little below the extremity there is a mouth, which terminates a canal, apparently serving for an œsophagus, and traversing the whole length of the body, pl. 4. fig. 2.

If

number contained from one to ten. I saw one with a single egg, so large as to distend the body in the middle very much. Whether these substances are eggs or fœtuses is uncertain: they are probably the latter included by a fine integument; and I rather suppose that the body of the mother bursts to give them an exit or immediately after they are produced. It is much more difficult to isolate an animal that almost never moves than another that is continually traversing a fluid. Sometimes I did succeed in isolating sloths with eggs in watch-glasses. When the young appeared, the parent was no longer visible, at least I could not find it; and they were remarkably small in comparison to others. There was no sand where they were found; their natural abode was among the particles of vegetable substance. I did not succeed in reviving them; but their scarcity prevented me from making repeated and accurate experiments. This, if it is a sloth, may be named *Tardigradus Octopdalis*, and that in the text *Tardigradus Italicus*.—T.

If the ſand is quite dry, they are ſeen motionleſs, dried up, and generally bent into a ſpiral. When conſiderably ſprinkled with water, they ſoon exhibit ſigns of life. The tail firſt commences a gentle motion, bending and turning in different directions: the head then moves, and afterwards the reſt of the body: ſo that the whole animal ſoon becomes animated. Whence it reſults, that the ſame degree of humidity is not required to animate this ſpecies as for the wheel animal and ſloth, which do not revive unleſs completely immerſed in water. The eels do not change their place: they only extend, contract, turn, and bend. If the ſand is thoroughly wet, their activity and rapidity of courſe is as great as that of the eels of vinegar. Provided they have water, they live long in watch-glaſſes: if there is ſand at the bottom, they ſeldom quit it, always moving about the grains and puſhing their heads among them, which would induce us to ſuppoſe that they do ſo in ſearch of food, for ſome more minute and delicate particles are tranſmitted by the mouth to the œſophagus. Notwithſtanding they have been long kept in glaſſes, I never ſaw them propagate.

When the water evaporates, they die; but they reſiſt death longer than wheel animals and ſloths. A ſmall degree of motion remains ſeveral minutes after evaporation. When dead, the figure of the body is changed: the length is contracted, and the breadth diminifhed. They infenfibly refume their original fize on humectation, and animation returns. There are conditions neceffary for refurrection: When the eels are in fand, a quarter of an hour is fufficient for recalling them to life; but in pure water, there is a great difference according to circumftances. If only the firft or fecond time of revival, there is not much difference in the time required for refurrection; but, in proportion as the number of refurrections increafes, the time neceffary for revival always becomes greater: an hour at leaft, and fometimes more, is required for the fourth; for the fifth ftill longer, and fo on for the reft. The frequency of refurrection in pure water, as in fand, is limited, like that of wheel animals and floths. The eels die for ever at the feventh or the eighth, or, at moft, the ninth refurrection; and, although moiftened again, they revive no more. Part of their rapidity and activity is loft in each refurrection, fo that the laft is but a fimple change from immobility to languid contorfions of the members.

Here then are three fpecies of animals, inhabiting the fand of roofs, which nature has permitted to revive after death. Thefe three are the only inhabitants of this fand, at leaft I do not think I have ever feen other animated beings there, having

ing a permanent abode. They are not the only animals, however, that enjoy the privilege of resurrection; others also possess it: among these, the celebrated eels of blighted corn deserve to be particularly mentioned. All the world knows that Mr Needham is the author of this famous discovery. Examining the internal surface of blighted corn, he saw with agreeable surprise that it was composed of minute eels, which, on being wet, acquired motion, and gave certain indications of life. Their immediate resurrection, as he has observed, takes place when the ears are gathered still fresh and humid: if they have been gathered some time, and have lost their humidity, maceration is necessary; nor will this always be sufficient for resurrection; it is even requisite that the eels remain a given time in the water. When allowed to dry, they become motionless, and recover life on humectation. But what chiefly surprised the author of the discovery was, after having preserved the blighted corn for two years and more, the same phenomena were observed anew when it was wet.

The fact was too wonderful for others not to endeavour to ascertain it. It has been corroborated by several good observers, such as the illustrious translator of Mr Needham's work, where this discovery is spoken of (1); by the Count Ginnani

(1) Nouvelles Observations Microcopiques.

Ginnani (1); but by Baker in particular, in his excellent treatiſe on the *eels of blighted corn* (2). Among other things, he has ſeen the reſurrection of eels taken from grain that had been dry four years. This obſervation he made before Mr Folkes, then Preſident of the Royal Society, and other friends. But he witneſſed a reſurrection much more wonderful, which was effected after a far longer time. In 1771, he had ſome blighted corn which he had got from Mr Needham in 1744. In his experiments, reſurrection ſucceeded perfectly at the end of twenty-ſeven years (3).

In ſhort, there is not at this day any profeſſor, any amateur of natural hiſtory, particularly in Italy, who does not take pleaſure in amuſing himſelf, and gratifying the curioſity of his learned friends, with theſe admirable reſurrections. For this reaſon, I judge it needleſs to ſtop and prove their reality by new facts, and to ſpeak of the origin and generation of the eels; for we know that this, which is a moſt eſſential part of their hiſtory, has been amply elucidated by the learned labours of Italians. The reſults of ſome of my trivial obſervations only ſhall be related, which will

(1) Delle Malattie del grano in erba.

(2) Employment for the Microſcope.

(3) Journal de Rozier.

will both ſerve as proofs of their hiſtory, and are analogous to thoſe we have given of other reſurgent animalcula.

The external colour of a grain of blighted corn, that has been kept ſome time, is like ſoot: if broken, the internal ſubſtance conſiſts of a dry whitiſh matter, which, examined with the microſcope, changes to a maſs of long eel-ſhaped corpuſcula. They are not only exceſſively dry, but lifeleſs, and ſo confuſed and confounded together, that it is extremely difficult to ſeparate them without rupture.

If the grain has been ſome hours infuſed in water, and the extremity adroitly cut off, without injuring the interior, and then preſſed with pincers, a parcel of minute eels are ſeen paſſing through the hole, juſt like a bit of paſte drawn into a thread. When dropped into water, they ſcatter; and, falling to the bottom, are extended as ſo many ſtraight lines, or a little curved, and remain in this poſition until revival.

Such variety has occurred in the time for revival, computing from the moment of humectation, that I have never ſeen the ſame thing twice. The anguillæ of ſome grains were reanimated in three hours or leſs; and others, in four or five. Some required twenty hours or more; and ſome, complete days. All thoſe of the ſame grain were not reanimated at once; ſometimes

two

two days intervened between the animation of the first and last. The whole do not revive: some are disfigured and lacerated, part are always so; but some apparently entire and unhurt remain motionless. Resurrection is affected by the state of the weather: it is accelerated by heat, and retarded by cold; but here also are irregularities.

It may be useful to describe the symptoms which announce the revival of the eels. The first indication of returning life is a deviation from the straight lines their dead bodies formed: the head and tail begin to curve, though the rest of the body continues in a straight line. Sometimes the two extremities do not bend: the body only becomes a little arched in the middle. One will gently oscillate, while the other does not move: sometimes they approach each other until a circle is formed by the extremities touching. One extremity will rest on the other, or glide over it, or both are entwined together: sometimes the whole body is rolled into a spiral in more or fewer, in wider or narrower volutions. These bendings, arcs, oscillations, circles, twinings, glidings, volutes; these contortions are formed and destroyed, and repeated at first very languidly, then in a manner more lively and perceptible. This strange variety of motions, with others which it is unnecessary to describe, continues during all the time they live. Whence it appears they have nothing that may properly be called

called progreſſive motion, which conſtitutes a difference between them and the other reſurgent animals. They never riſe in the water, nor do they crawl on the ſides of the veſſel; they conſtantly remain at the bottom, appearing like a pellicle or ſpot darker or lighter according as they are more or leſs numerous.

If the water fails, whether by evaporation or otherwiſe, the eels gradually become lifeleſs, and motion ceaſes when there is no more water. The other three kinds of reſurgent animals have the prudence to fly the places where the water dries; but the eels continue in the ſame ſpot without attempting to eſcape.

In ſeveral hours, they become very dry, and adhere to the ſubſtances below ſo tenaciouſly, that it is difficult to ſeparate them without breaking: when wet, they ſeparate eaſily, eſpecially with the point of a needle. They ſoon ſoften, and, becoming pliant, it is evident they are of a gelatinous conſiſtence: and an iron inſtrument cannot touch them without injury. This, at leaſt, happens while alive: when dead ſome days, they are ſtill very fragile, yet have more coheſion than one would think; they reſiſt the point of a needle, and do not ſuffer from a drop of water let fall from a conſiderable height. If dry only a quarter of an hour, the contact of water reanimates them; and in a little they become as

vivacious

vivacious as before. Urine, ſalt water, and vinegar, produce ſimilar effects, though fatal in other circumſtances, as we ſhall ſee. When dry during ſome days, they require a full hour for revival. If one has patience to wet them, and allow them to dry, death and reſurrection will be ſeen in an important limitation: which is, the oftener humectation is repeated, the leſs the number of reſurgents will be, and the longer time required for revival. I had a number of lively eels in a watch-glaſs, the firſt time they were revived: one thouſandth part did not revive the eleventh time, and the ſeventeenth there was not one. I have often repeated this important fact, and always with the ſame conſequence, except that the reviving eels either went beyond the ſeventeenth time, or died before attaining it. Not only wheel animals, ſloths, and the minute eels of roofs, but alſo thoſe of blighted corn, enjoy the property of reſurrection circumſcribed within certain limits, beyond which it is loſt. The body to revive muſt be entire. Eels, cut into two or more parts, though often wet, and remaining long in water, never exhibit any ſign of motion. All ſenſation is loſt on diviſion in two, after a ſlight univerſal vibration or convulſion of the body.

I have ſubjected the eels, as well as wheel animals, to different experiments, and firſt to electricity, uſing Franklin's battery. Thoſe alive died

died instantaneously, and those dead at the time, lost the property of resurrection. This did not surprise me: for almost all were broken or disfigured by the traversing shock. There was a difference in the results, if the blighted corn was subjected to the same experiment; few revived when the grains had been previously macerated: if the grains were dry, many recovered life.

As salt water, urine, and vinegar are unfit for reviving the anguillæ, at least if they have been only a short time dry, so are they fatal to them when revived; not so instantaneously, however, as to other animalcula, for eels will move in them some hours after immersion.

A vacuum does not prejudice their resurrection, whether the first time after proceeding from the grain or in future: only, resurrection is not so soon accomplished as in the open air.

The heat of the sun or the fire at 140° kills them in several hours: motion and life are almost immediately destroyed at 144° or 149°. Heat is a more powerful agent on wet grains than on dry. The observer will commonly have many eels from grains that have suffered 138°: most part are killed at that degree if the grains are wet.

When freezing water becomes solid, the eels cease to move. Cold 8° below 0 does not destroy

ſtroy the reſurgent property, and life returns on melting of the ice.

Thoſe who have never ſeen the eels of blighted corn will find them deſigned, fig. 3. 4. 5. Pl. 4. as they ſwim in the fluid before a magnifier not very powerful. Seven blighted grains are repreſented of their natural ſize and figure, fig. 6. and three magnified, fig. 7.

Plants are beings ſo analogous to animals, that he may be excuſed who has defined them *rooted animals.* In the works of Vallisneri, Buffon, Bonnet, and, laſtly, of the Abbé Corti, may be ſeen the numerous and various traits of analogy between theſe two claſſes of organiſed beings. The ſubject of which we treat preſents a new analogy: for as different animals revive after death, ſo do many plants ſpring again after they have periſhed. It would be departing from my plan was I to ſay as much of them as I have ſaid of animals: and I ſhall be content with mentioning two, the *noſtoc* and *tremella.* The noſtoc, ſo named by Paracelſus, is a terreſtrial plant, whoſe ſudden appearance in places where there was no ſign of it before was conſidered by the ancients rather as a prodigy of heaven or earth than as a plant. Thus they denominated it *Heaven's flower and Earth's flower.* It is ſeen in all ſeaſons, but particularly in ſummer after heavy rains. Though it ſprings in every ſoil, it prefers

meadows

meadows, arid lands, and ſandy valleys. The colour is a browniſh green; the figure irregular, and reſembling a leaf careleſsly folded. When ſeparated with the fingers, ſome reſiſtance is felt, ſuch as one feels on tearing a young leaf. If a ſudden drought happens, the noſtoc contracts and dries, remaining only a ſhrivelled fine thin ſkin. If a ſudden and heavy rain falls, it again becomes green, and reſumes its original ſize. Therefore the noſtoc, as Reaumur, who has furniſhed me with this intelligence, obſerves, is a plant of a ſingular kind, ſince it recovers life after being in a ſtate which to others would be permanent death (1).

 The

(1) I have never been able to find the *tremella noſtoc*; but I have made a few experiments on ſome plants of the ſame genus. There is one, of a beautiful yellow, which in damp or wet weather appears on decayed wood. The largeſt, when in full vigour, that I have met with, is about an inch and a quarter long, and about an inch high, of an irregular figure. I believe it is the *tremella diliqueſcens*. When allowed to dry, it becomes of a deep brown colour, not an eighth of an inch in ſize, and very hard. By wetting it, the full ſize and figure are re-acquired. It may be repeatedly dried and moiſtened without injury. I think it is longer of expanding and of the expanſion being complete according to the period of deſiccation and the number

The same privilege is possessed by the tremella, which is an aquatic plant, placed by botanists in the class Conserva (1). If it chance to be in a vessel where the water fails, it dries and loses its verdure; but water being supplied, it soon recovers its original state. Nature does the same as art. I have seen, from the beginning of July [illegible] till [illegible] ber of revivals; but this fact merits farther investigation. After eighty days desiccation, it immediately revived. —T.

(1) Naturalists have lately disputed much concerning the nature of Confervæ. By chemical analysis, they give products chiefly peculiar to the vegetable kingdom: 1. They give out an acid, and a small quantity of ammoniac, combined with pyromucous acid; 2. They contain muriat and carbonat of potash; and, although soda may be found in them, we know that it is also found in many vegetables; 3. The quantity of ashes is rather an indication of their vegetable nature. This analysis was made by Vauquelin, who concluded from it that confervæ had a greater relation to the vegetable than the animal kingdom. M. Decandolle has compared the sentiments and discoveries of modern naturalists on the subject; and, in a judicious memoir, he thinks "Confervæ are neither intermediate beings between animals and vegetables, polypi, nor *polypiers*; neither are they aggregates of animalcula, but real vegetables, analogous to tremellæ, fuci, and lichens." *Rapport sur les Conserves*, Journal de Physique, tom. 54. p. 421.

till the end of October, a ditch for watering land covered fifty times with the beautiful verdure of the tremella, and seen it as often disappear, when there was no water. Colourless hairs or wool only were visible at the sides and the bottom, which, the microscope shewed me, consisted of the tremella dry and dead.

What can be the reason why these animals and plants are thus privileged, in comparison to many others which, perishing once, perish for ever?—Shall we perhaps ascribe it to the simplicity of their structure? But this opinion or conjecture does not seem well founded. There are many animals that never revive, whose structure is as simple, or even more so, than that of the resurgent animals. Are not many infusion animalcula, which are composed of a simple aggregate of vesicles, undoubtedly less complex than wheel animals, which are provided with vessels, wheels, intestines, and ovaries? Yet they do not recover life when once it is lost. Simplicity of structure would even seem an obstacle to their resurrection; for the simple membrane of several species bursts on evaporation of the water: the animal is dispersed, and reduced to an unconnected and disordered heap of fragments.

The arm-polypus is no less simple than the animalcula of infusions, being composed but of a granulated gelatinous skin. If simplicity of struc-

 ture

ture influenced the resurrection of animals, the arm-polypus would certainly be one; and it seems so much the better adapted for resurrection, as it continues alive notwithstanding every method has been taken to destroy animation. It is demonstrated, that these polypi sustain no injury by being turned several times outside in, like a glove, or by being cut asunder. If the head is cut off, a sort of hydra with many heads arises, each of which receives food by a different mouth. If these new heads are cut off, new hydras spring up; and each head creates a polypus fit for the formation of more hydras. In short, every particle, even the smallest fragment of a polypus, unfolds and becomes a new polypus. If an animal so mangled and lacerated does not die, will it not be very credible that, only being allowed to remain dry, it may still retain the faculty of resurrection? But facts prove the reverse. The arm-polypus always dies when the water evaporates; and this happens equally whether it is immediately exposed to the air, or lies concealed among its native sub-aquatic herbs. When the water is almost exhausted, the arms are retracted into the animal; it contracts within itself, and dies. It never recovers, though water is copiously supplied. I speak of the arm-polypus, for it is the only species I have been able to find, and is the smallest of Trembley's arm-polypi.

Next to polypi and infusion animalcula, according to my description, the organization of the sloth seems to be the most simple. We may say the same of the anguillæ of tiles and blighted corn, two species of serpentuli, which may properly be classed with so great a number of the inhabitants of fluids from their organization. Under the tremella in water are often found minute eels very like those of tiles in size, shape, and simplicity of organization. I have frequently had the curiosity to let them dry, by the water evaporating: all endeavoured to conceal themselves where the filaments of the tremella were thickest; and, when evaporation was complete, they perished, remaining partly entwined among the filaments, and partly heaped above one another. If immediately wet, they revive; but never, if a few minutes elapse.

The eels of vinegar give the strongest evidence of vigour. Though they continue motionless when the fluid fails, and are apparently dead, they recover life and action, if wet, after a quarter of an hour. Sometimes I have succeeded in reviving them after half an hour. I do not call this resurrection: if it was such, I cannot see why it should not succeed anew when wet with vinegar, in even a longer time. We may rather say, they do not die so soon as the eels of the tremella, and many other insects left dry:

life, though suspended, is preserved, and appears on humectation.

I can discover no greater simplicity in the tremella and nostoc than in many plants that do not revive. Let us throw a hasty glance on the truffle. What vegetable is more simple? No roots, tendrils, or fibres, internal or external; a substance equally compact and uniform throughout, only interrupted by veins similar to those winding on some species of marble. It has no analogous organization with other plants, either terrestrial or aquatic; yet truffles, after once drying in the air, do not revive if put in water.

These united facts prove the fallacy of those opinions which attribute the resurrection of animals to the simplicity of their organization. But to what other principle can we recur? for we are here constrained to proceed on conjecture rather than evidence and the view of truth. I shall suggest an hypothesis without engaging to support it. Haller's experiments demonstrate, that the vital principle of animals with a heart originally resides in the irritability of this muscle. His experiments are too well known to need repetition. In animals which have no heart, it is more than probable that the principle of life resides in the irritability of their muscles. This being admitted, if the state of the animals is such that the irritable nature of the heart and muscles is destroyed,

ſtroyed, ſo as to leave no hope of reparation, it is clear that the animal not only dies, but muſt always remain dead: if the irritability is ſuch that it may be re-excited, either naturally or by art, it is indubitable that the animal will paſs from death to life. It will not ſignify though it remains dead a long time, even for an age. The reader comprehends my idea. When wheel animals, ſloths, and the eels of tiles are deprived of water, their irritability is evidently loſt, and they die. Other animals, having once loſt this irritability, never recover it; but it is awakened in wheel animals, ſloths, and eels, and they return to their original life by humectation.

From the ſame principle, may we explain why in certain caſes theſe animals loſe the reſurgent property when expoſed to powerful heat or penetrating odours, or when ſome liquids and electricity act upon them. Such agents injure the muſcular ſtructure, as appears by the rupture of the body and deſtruction of the irritable power reſiding in it. This perhaps is the reaſon why frequent humectations prejudice reſuſcitant animals; for I have really ſeen it; and in particular obſerved, that the members of the eels of blighted corn were injured and lacerated by repeated humectation.

We muſt conclude from the whole, that as irritability reſides in the glutinous part of the muscle, this part of resurgent animals has qualities very different from the irritable parts of other animals, though we are profoundly ignorant of what constitutes the difference, because we are profoundly ignorant of what the gluten consists.

I wish to be sincere. A conclusion against the hypothesis may be deduced from my experiments. Irritability is recognised by its appearance, that is, on touching the muscular fibre with any stimulant, it contracts and becomes rigid. I have often stimulated the muscular substance of the eels of blighted corn and tiles, with an extremely fine iron point, and attentively observed the consequence. The muscular fibre always seemed to contract a little when touched; but I must acknowledge the same thing happened to the anguillæ of vinegar, and to other analogous animalcula, which do not enjoy the privilege of resurrection. There are even some aquatic and terrestrial vermiculi more irritable than the eels, since with the most gentle touch they contract and swell, until they become many times as thick and short. The objection is therefore confined to this: there are some animalcula which do not revive, though as irritable, even more so than those that do. But it does not affect my hypothesis, for the principle of resurrection is not placed in the greatest and most perfect irritability, but in an irritability which, after cessation, may be renewed by means of certain circumstances, though it otherwise appears to be less active than in other animals.

If this hypothesis does not seem fully applicable to plants, in what concerns their irritability, since we know only a very small number possessing that property, still it may be applied to what respects their organization. Dried plants in general do not recover life, probably because they are so much injured in drying as to become incapable of imbibing the juices provided, and converting them into their own substance. Thence do they perish, and are totally destroyed. If such disorder is not occasioned by drying, and the organic action of plants revives when they are softened, and resume their original form, it is undoubted that they will then recover their pristine verdure and natural freshness. This may be the physical cause why the tremella, nostoc, and some other vegetables revive.

This tract may be terminated with some reflections on those beings which we can kill and bring to life at pleasure. When presented to the mind we are astonished, because they are isolated beings: they form a separate class, and the ideas they suggest are adverse to those received of the animated world. But when it is proved by a series of innumerable facts, that all is gradated in nature,

nature, that these beings are connected with other beings, consequently that the isolations exist only in the general system, wonder should cease, or at least be diminished, since it only arises from our ignorance of the relations that connect the classes enjoying the privilege of resurrection with those that do not. This is not the only isolated fact which has existed, and at first been considered an exception to general laws. The works of Reaumur, Trembley, Bonnet, will communicate many. The exceptions appeared singular, because they were seen but in one instance. A plant, an animal of a new genus, or possessing peculiar properties, is the origin of such exceptions, till farther observation and solid experiment proves them adapted to several cases, either in the same circumstances, or under modifications, and proportioned as human industry has been applied to diversify the number of subjects, and render them so numerous, that they can no longer be called exceptions. The surprise that affects the mind from something new or extraordinary gradually diminishes and vanishes entirely. One or two examples will corroborate and elucidate these reflections on resurgent animals.

One of the most effectual methods to destroy animals is to cut them in pieces: nothing is more common or better known. To say this is a mode [illegible] of

have

of multiplying some species is affirming what has a fabulous appearance. Yet it is certainly the case with the arm-polypus; and shall we admit that the discovery is bounded by this animal? The scalpel need only be applied to others, to prove that the fact is wonderfully extensive. Thence are the reproductions of the earth worm, the boat worm, the fresh water worm, some leeches, sea stars, and nettles. While art effects prodigies on these species, nature prepares similar ones in silence. I speak of propagation by the natural division of the dart millepede, many races of club, funnel, and bell polypi, and infinite infusion animalcula. It is also found that nature sometimes forms not only two from a single animal, but even four, nay, a multitude so prodigious, that as many arise as there were atoms in the generating animal. The polypus does not terminate the wonderful progress of discovery. It is a chain passing from vegetables to animals, and leads to man. The tremella is the link connecting animals and vegetables; it is a real zoophyte. It has been discovered that the filaments of which it is composed divide spontaneously, and that a complete plant springs from each division (1). The polypus is joined to the tremella,

(1) *Adanson*, *Fontana*, *Corti*, *Desaussure*, and other philosophers, have discovered that certain species of tremella have

tremella, and is united to many races that divide like itself, which in a similar manner are linked to other species. Reproductions are not effected in the same way in all. One may reduce a polypus, a fresh water worm, a sea star, or nettle, to the smallest fragments, and be certain that each particle will reproduce itself. The earth and boat worm must not be cut in pieces so small when we wish to obtain reproduction. If a snail is decollated, a new head will germinate, but the severed head will not acquire a body. Water newts and frogs, while tadpoles, recover their tails and limbs when deprived of them; but if mutilated in other parts of the body, they perish (2). No warm blooded animal is yet known which reproduces itself when cut in pieces, but animals recover large pieces cut away. Duhamel has seen a ring of flesh cut to the bone reproduced in a chicken. Similar reparations are every day observed in the cicatrised wounds of men and animals: and we have certain evidence of the reparation of the

have a real progressive motion. From this some naturalists suppose that it is endowed with animality, or constitutes a link which connects the animal and vegetable kingdoms, though properly belonging to neither. Other naturalists think these species are real vegetables, but that their motion or progression is owing to some mechanical means.—T.

(2) Prodromo sopra le Riproduzioni Animali.

the tibia in a man (1). It is thus that the discovery of the polypus, which at first seemed to revolt against rules esteemed general, has extended to so many links in the animal chain. But has this success so great, this advance so rapid, exhausted the subject of reproduction? No, assuredly: it will appear but little known, if we consider how limited the number of animals on which our experiments have been made, compared with those on which we may make them. The element water is most favourable to reproductions. How many insects, worms, reptiles, zoophytes inhabit the briny waters of the ocean, the fresh water of rivers, pools, marshes, and ditches, which were never subjected to experiment, and which, from their great similarity, whether in the modes of life and propagation, or in shape and internal configuration with reproducing animals, are doubtless calculated to reproduce their parts?

Hermaphrodism, until the beginning of this age, has been considered more chimerical than true. Nature seemed to declare against it. Yet in how many hundred animals has it not been found by the diligence of modern naturalists? This admirable property passes by degrees from one species to another. The polypus is a perfect hermaphrodite

(1) The celebrated Bernard Moscati, surgeon in Milan, who was an eye witness, told me this fact.

hermaphrodite without ſex: it multiplies both by diviſion and ſhoots. The puceron of plants is leſs an hermaphrodite; it has a ſexual diſtinction; and although it propagates without copulation during ſummer, at the end of autumn it is obſerved to copulate. Earth worms, ſhell ſnails, naked ſnails and many ſpecies of ſhell fiſh are leſs hermaphrodites: each is at the ſame time male and female, but inſufficient alone for generation. They give and receive; they fertilize and are fecundated.

The diſcovery of reſurgent animals is far from being as extenſive as that of reproduction or hermaphrodiſm; but this ariſes leſs from the ſcarcity of ſuch beings than the rareneſs of the philoſophers who have entered on this branch of natural philoſophy. Leeuwenhoeck was the firſt who, by his reviving wheel animal, drew the curtain from before theſe objects: and ſeems ſurpriſed at a fact which was unique and unexampled in nature: and, indeed, no one thought there was another creature in all the animated world poſſeſſed of this prodigy. But ſince the profound reſearch of more modern naturaliſts has diſcovered others, I doubt not that the number of theſe wonderful beings will increaſe as the ſtudy is cultivated.

The ſame gradations perceptible in hermaphrodites and in reproducing animals cannot be expected in the reſuſcitant. There may be a greater or leſs

leſs degree of reproduction: an animal may reproduce more or fewer organs: and an animal may be more or leſs an hermaphrodite, if hermaphrodiſm is taken in a comprehenſive ſenſe. Both theſe caſes are poſſible, and, as we have ſeen, are realiſed by facts. But we cannot reaſon thus on the animals reviving after death; we cannot ſay an animal dies more or leſs, or that its reſurrection is more or leſs. Death and reſurrection are two indiviſible acts, ſo much, that neither has any gradation in the animals hitherto known; and it cannot be expected in thoſe we ſhall hereafter diſcover. It is not that theſe animals have no particular gradations among themſelves, or are not connected by them with others of the ſame kind: nor are they difficult to be obſerved. We have ſeen the ſtate to which ſome animals are reduced by cold: it cannot properly be called death, for there is a thread of life, a leſſer life, which may connect them with reſurgent animals. The verdure of moſt plants is loſt in winter: they have little ſap, and it is motionleſs: they ceaſe to imbibe nutriment, to grow, to multiply: thence are they in perfect inaction. Beſides inſects, how many animals do we ſee with this leaſt degree of life, not excepting ſome warm blooded, and among them birds? I am far from coinciding with Guaguin in this deſcription of Muſcovy: 'Populos quoſdam in Lucomonia Regione

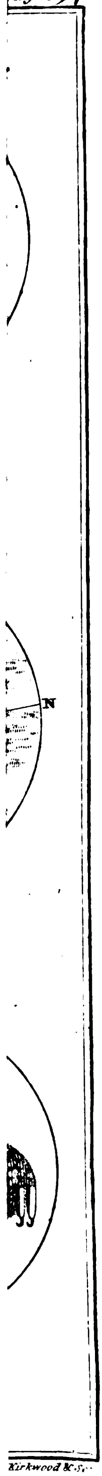
'Ruſſiæ

'Russiæ habitantes quotannis vigesima septima Novembris die, ut solent hirundines, et rana, sic et ipsos præ frigoris hyemalis magnitudine mori: postea redeunte vere vigesima quarta Aprilis die denuo reviviscere.' At the same time, we cannot deny that man himself may sometimes be in a similar situation with animals overcome by cold, as when he has been immerged in water, without entirely ceasing to live. I will not affirm with some philosophers, because there is neither pulsation nor respiration evident in the body, that pulsation of the heart and circulation of the fluids are suspended; but I would rather think with Haller, that these motions are only too faint and obscure to be externally perceptible. Examples of this we have in some animals half drowned, where a degree of motion is always sensible in the heart and in the blood. The life of man and animals half drowned cannot be more feeble; and we may regard it as another point in the passage from the resuscitant to those animals which do not revive.

There are other two states very similar to the death of resurgent animals. One is the state of the embryo in a fecundated egg before it has experienced the heat necessary for expansion: life is incomplete: there are but the rudiments of life. The other is the state of a chrysalis among insects. When the caterpillar has lost its natural form,

form, it assumes that of a shapeless mass, without the vestige of feet or wings: it ceases to feed; indeed to eat would be impossible, for the organs are wanting: it has no longer loco-motion; and we should really believe it dead, but for some inflection and contortion of which the posterior part is susceptible. Apparent death is still more sensible in the nymphs of many worms; no stimulus can awaken any symptom of life or sensation.

Thus there are situations in nature somewhat similar to the state of dry resurgent animals: and these situations may be protracted or abridged at pleasure the same as with resurgent animals. That torpid animals may never awaken from their lethargy; that the embryo may never expand in the egg; that flies and butterflies may never proceed from the nymphs and chrysalids, nothing is requisite but to keep them continually exposed to cold. The reverse will happen on exposure to heat. Probably there are gradations of connection more immediate and more direct between the animals that revive and those that do not. Life, however feeble and obscure, is always life: between it and death there is a distance as great as between existence and non-entity. An animal, whose life might be suspended from an impediment to the mutual action of the solids and the fluids, would be the link connecting the least degree of life with death. Such an animal is yet unknown: but we ſhould not deſpair of finding it, ſince ſo many are diſcovered which connect and more intimately unite the animated chain. Let us only reflect, that natural hiſtory is yet in its infancy, and that our diſcoveries are nothing, compared with what we have yet to diſcover.



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OBSERVATIONS AND EXPERIMENTS

ON

THE ORIGIN OF THE PLANTULÆ OF MOULD.

THE mould, which I have examined, and intend simply and briefly to defcribe, fprings on moiftened bread, apples, pears, melons, or gourds, beginning to fpoil. It may be divided into two kinds, the one very fimple and eafy to be obferved: the other complex and intricate, which can only be underftood by a generic defcription. Let us begin with the former.

One fpecies is without branches, and each filament bears a globule at the fummit, pl. 5. fig. 3. another is ramofe, but with this difference, that fome plants have a globule at the vertex, while others have none, fig. 2. Thefe globules fhall always be termed the minute heads of mould;

but

but with reſpect to them there is a remark neceſſary. Without the microſcope, we ſhould ſuppoſe them perfectly ſpherical, and even with it they appear ſo if viewed from above; but when examined below, that is where the ſtalk is inſerted into the head, we obſerve that all, or the greater number, are ſhaped like muſhrooms, or, to ſpeak more philoſophically, they are real muſhrooms. Two filaments, with globules, are repreſented, fig. 7. and highly magnified, fig. 8. A kind of mould ſometimes grows on pears, which is actually a ſhrub in miniature, univerſally adorned with ſpherical heads and muſhrooms, fig. 6.

Ramous mould is often attached to vegetable ſubſtances without the aid of roots; but mould wanting branches almoſt always has roots originating from a round corpuſcle, from which the filaments or ſtalks of mould ariſe. It is ſingular, that in proportion as every root gives origin to a greater number of ſtalks, ſo are the filaments it ſends forth below more numerous in proportion. A degree of reſiſtance is felt on tearing mould from the ſubſtance where it ſprings, which is the conſequence of the roots being well fixed. When torn up, they appear very crooked, while the ſtalks, that have not ſuffered from the impreſſions of the air, are perfectly ſtraight. Many of theſe are of an equal thickneſs throughout, and become a little ſmaller towards

wards the top. The defcription will, perhaps, be better underftood by fig. 3.

Mould is at firft of a moft beautiful white; it next acquires a yellowifh tint, and at laft blackens; however, the heads grow much blacker than the ftalks and branches. The origin and increafe of mould is nearly in proportion to the heat of the atmofphere; its appearance and vegetation are never more fudden than during the heats of fummer. A fubftance which does not exhibit a fingle ftalk of mould at night, will often be covered in the morning by mould, which has then attained its full fize and maturity (1). It does not increafe fo much in height as in thicknefs, and the heads, already black, are always of larger fize than when they were young and white.

Mould never rifes fo beautiful and vigorous as when vegetating under fome veffel or receiver, providing the communication with the external and internal air remains. The reafon is evident.

 The

(1) Bradley remarks that the feed of mould has vegetated in three hours, and in about fix more, the plant had attained its perfect fize, *Philofophical Tranfactions*, 1729, p. 491. However, fome mould takes much longer time. Nay, it is often very long before mould is produced on a fubftance, either from abfence of the feeds, or the fubftance not being well adapted for its vegetation.—T.

The ſtalks being very fine near the ſummit, and bearing on the vertex a round corpuſculum, which oſcillates the filament by its own weight, as the ear of corn occaſions oſcillation of the ſtalk, we may eaſily perceive that every breath of air, however gentle, will bend, break, and deſtroy their moſt delicate texture, which does not enſue when the moulding ſubſtances are put under a receiver. Beſides, their humidity is better preſerved, a condition moſt eſſential for the production and increaſe of mould. In the courſe of theſe obſervations, I have always uſed receivers. The prejudicial conſequences of agitated air are viſible, fig. 1. which repreſents two ſpots of mould with heads, viewed with the naked eye when taken from under the receiver, and expoſed a ſhort time to the action of the air; their natural direction is loſt, and the ſtalks bend in various directions.

Some ſubſtances, put in a ſituation to acquire mould, gradually diſſolve into a kind of acid fluid, wetting the circumjacent parts; it is preciſely here that the mould hitherto mentioned ſprings. A ſmaller portion of moiſture likewiſe exhales from the ſame ſubſtance, which adheres to the inſide of the receiver, in the form of a pellucid aqueous veil, and increaſes ſo conſiderably as to form large drops that run down the ſides of the receiver in winding ſtreams: an equal quan-

tity

tity of mould grows on theſe ſtreams, as may be obſerved if the glaſs receivers are tranſparent.

But the other kind of mould, which in the beginning we have ſaid is very complicated, always ſprings on the immediate ſubſtance of the vegetable itſelf, and particularly prefers gourds and moiſtened bread. When theſe begin to acidify and corrupt, a thick white covering of mould appears on the ſurface, which in a few hours is an inch high, and, when ripe, three inches or more. This is different from the laſt ſpecies, which, at complete maturity, is ſcarcely half an inch high. We have already obſerved, that each plant may be examined ſeparately, and without confuſion, and that the examination may be diſtinctly continued until maturity; but it is otherwiſe with the mould before us; for it is impoſſible to examine each plant diſtinct and ſeparate. The immenſe number of them when the mould begins to ſpring, the interlacing of the ſtalks and branches which entangle and interweave in a hundred different ways, is a complete obſtacle to it: the eye is abſolutely confounded, viſion is confuſed, and can only diſtinguiſh an intricate aſſemblage of ſlender filaments, which, as vegetation advances, become more and more deranged, therefore a general deſcription of the whole together muſt be given. The ſtalks, torn from the ſubſtance where they ſpring, ſeem to have no roots.

roots. Numerous twigs and branches proceed from each, which are frequently of as great diameter as the parent ſtalks whence they originate. While the ſtalks ſucceſſively vegetate and extend, ſeveral groups of ſmaller branches ſpring out laterally, with minute heads at the vertex. Theſe are partly fungi-form, partly globular, and, as the ſtalks riſe, they blacken and come to maturity. Additional new ſtalks next appear; and multiplication continues ſo long as the mould vegetates. This little foreſt of filaments is terminated by moſt minute points, and wholly covered with black globules.

Thus does mould originate and arrive at maturity. We need not inquire whether it is a real vegetable: it evidently is ſuch, by the obſervations I relate. But theſe vegetables, or microſcopic plants, do not poſſeſs two properties common to other plants. Ligneous and herbaceous plants, expoſed to natural light, always tend to take a direction perpendicular to the horizon. They endeavour to attain it when an impediment intervenes. The experiments of M. Bonnet are excellent, as may be ſeen in his work *Sur l'uſage des Feuilles*. But we do not ſee this tendency to perpendicularity in mould; for, although many ſtalks are perpendicular, it is not eſſential to their nature, and innumerable others are in a different direction. If a plant grows in the rent of a wall, though it firſt appears

appears in a horizontal poſition, it ſoon riſes perpendicularly, and ſo continues to grow. It is otherwiſe with mould. I have often cut a piece of melon, gourd, or bread, into a cube: mould vegetated on the four lateral ſurfaces, and the ſtalks had conſtantly every other than a perpendicular direction (1).

The other property, which was diſcovered by the celebrated Geneveſe naturaliſt, is the tendency of plants to turn towards the light. In addition to the facts he recites, which are ſufficient to aſcertain the property, I have frequently obſerved it in legumes growing in infuſions, ſhut up in a preſs: the plants always bent towards a chink that admitted a ſlender ray of light; and if this chink was ſtopped up, and a new one opened in a different part of the preſs, the plants abandoned their original direction for this new one. I could never diſcover that light had the leaſt influence on mould.

If ripe mould is ſhaken, a kind of black duſt falls from it, which the celebrated botaniſt Micheli has ſuppoſed the ſeed of the plantula; but the

(1) This may generally happen; but I have, in many inſtances, ſeen an evident tendency to perpendicularity, and vegetation in a ſtraight line. Deviation from it may perhaps be chiefly occaſioned by the extreme delicacy of the ſtalk.—T.

the elder Dr Monti, alſo a very eminent botaniſt, has called the truth of his opinion in queſtion, and rather inclines to think that mould ſprings by ſpontaneous generation. Before diſcuſſing this matter, which is ſo intereſting, it is proper to examine where the duſt is found, which renders it neceſſary to make a brief analyſis of the heads of mould, which can only be done by ſeeing it ripen. Before maturity, the heads are of a whitiſh and yellowiſh colour, the ſurface very ſmooth, and they are firmly attached to the ſtalk. Broken with a fine needle, they ſeem membranaceous, and full of a granulated ſubſtance. If, inſtead of being broke, they burſt, a number of moſt minute round ſeeds ſometimes come out: theſe are found both in the ſpherical and fungiform heads. When they blacken, the appearance changes; the ſurface ſeems unequal; it is lacerated in ſeveral places, and reſembles a parcel of black rags. Many ſeeds are ſeen when opened; but young mould has white ſeed, and old or ripe mould has black. By letting a drop of water fall on the heads, the ſeeds are ſeen more diſtinctly and in greater abundance. On contact with the fluid, or a little afterwards, the heads burſt and ſcatter a cloud of ſeeds around; ſo that I may affirm, without danger of exaggeration, a thouſand are in each head. The unripe heads do not open in this manner when wet; they remain

main entire; and it muſt be remarked, that the ripe ones are not altogether decompoſed. Both in the round and fungiform is a little head in the centre, which continues adhering to the ſtalk; it is cinder-coloured, and does not appear black like the exterior. It is difficult to be detached from the ſtalk, but, with gentle preſſure, a ſmall jet of ſeeds, reſembling thoſe I have deſcribed, is raiſed; after which, the central head becomes a dry, empty ſkin.

If the heads, black and ripe, are opened by means of water, quantities of ſeed ſo great eſcape, that they adhere to the plants, and the ſtalks particularly, in ſuch a manner that one would ſuppoſe the exterior compoſed of ſeeds alone, were they not previouſly ſeen in a different ſtate. The deceitful appearance of two plants in this ſtate is repreſented fig. 9. One is completely covered with ſeed; the head is magnified, and great part of it alſo covered. Three ſtalks are repreſented fig. 4. The whole ſeeds of one head are expoſed; another head is partly covered by the integument; and the third wholly ſo.

A quantity of this duſt conſtitutes the powder which blackens the hands when mould is touched; and it is conſidered real ſeed, by the celebrated Florentine botaniſt. To aſcertain the truth, he had recourſe to a method apparently deciſive, which was ſowing the duſt. He ſtrewed ed it on ſome vegetable ſubſtances, and ſaw them covered with mould. But the Bologneſe Profeſſor repeated Micheli's experiment without finding it ſo concluſive; the vegetable ſubſtances being equally covered, though no duſt was put upon them. Thus the queſtion was undecided, for I do not know that any other perſon has attempted to ſolve it. Perhaps I ſhall be taxed with preſumption for ſaying, that, by means of experiments analogous to thoſe of Micheli, it has been in my power to aſcertain the fact, but experiments much more numerous, diverſified and connected, which I muſt be permitted to narrate.

Two pieces of moiſtened bread were taken, as ſimilar to each other as poſſible, and from the ſame loaf, ſo as to be perfectly equal; and I endeavoured to attain the ſame equality in all the reſt of my experiments. One piece was ſtrewed with duſt, taken from a quantity of ripe mould heads, in ſuch a manner that the ſurface was faintly blackened: the other was untouched, on purpoſe to compare the production upon each. This was done in ſummer. Next day, the *ſown ſubſtance*,—but, for brevity it may be obſerved, that by this is meant the ſubſtance of whatever nature covered with duſt; and by *unſown*, the other vegetable ſubſtance not covered,—on the ſown ſubſtance, I ſay, a ſhade of mould appeared, whereas on the unſown was none. Before the

the third day, both were covered with mould; but that on the sown substance was almost double the height and thickness of the mould on the other. Both species were the same, and perfectly similar to what had produced the dust. On the fourth day, the mould of the unsown substance, though not so thick, was equal to the other in height; it was even higher the following day, but afterwards continued to become thinner. These experiments were repeated eleven times on moistened bread: twice the mould became equally high and thick on both substances, and nine times it was higher on the unsown one, but thinner. It constantly sprung first on the sown substance.

Having collected a great quantity of ripe dust, I thought of varying the portions scattered on moistened bread. The consequences were new: When the quantity of dust was very small, there was almost no difference in the height and thickness of the mould on either substance sown or unsown; however, the thickness increased by sowing a greater quantity, and it was never so thick as when liberally strewed over the bread: then the mould was a real covering; but in proportion as the thickness augmented, the height diminished. The experiments were repeated again and again on apples, pears, and gourds; and

and all the results were, to a certain degree, more or less similar to what are related.

We may, in the first place, deduce, that sowing the dust accelerates the production of mould; secondly, The thickness is increased; thirdly, The height is less. Considering these facts with respect to my object, it seems that the second proves the dust to be the real seed of the mould; for more abundant production arises from scattering it. If the thickness increases in proportion as the quantity sown is augmented, it is natural to suppose the superabundance of mould on sown substances an effect of the dust, or rather of the minute seeds sown; and that all or most part of the mould originates from them. This being the case, we cannot be surprised if mould on sown substances is not so high as upon unsown; for, the plants being more numerous, each can not imbibe the same nutriment from it as may be derived from that which is unsown, where there are fewer. The same also succeeds with other plants, which are smaller and shorter in proportion as they are more crowded together. The first consequence deduced from these facts demonstrates, that the production of sown mould is earlier than that of unsown. I have thought it might be because the substances spoil sooner; since it appears that, by means of the dust, they sooner contract that principle of acidity and putrefaction, on

which,

which, as we have already remarked, the origin of mould depends.

The experiments were diversified. Sometimes I covered a half, sometimes two thirds, or one, of a slice of bread, an apple, a pear, or a gourd, without touching the other half. The half, two thirds, or one third, were just in the condition of the sown substances. I likewise made another experiment. After covering half a slice of bread, apple, or gourd, with dust, the surface sown was applied to another surface similar but unsown, and both left in this state several days. On the whole sown surface a veil of mould appeared, the vegetation of which had ceased, because it was spoiled by the substance applied: but no vestige of mould was seen on the unsown part. Thus the hypothesis is corroborated, that the dust is the real seed of the mould, because that produced on the places sown was exactly of the same species with what had afforded the dust.

Notwithstanding all these plausible and repeated experiments, I was not satisfied. Is it not possible, said I to myself, that this dust only renders the soil more fertile, so that it will produce a greater quantity of mould, as the earth fertilized by foreign matter will produce more plants? Certainly it was not impossible; and, wishing to proceed with philosophic strictness, I judged myself obliged to realize or remove the possibility: for

for which purpose it was proper to cover moulding substances with dust taken from different vegetables, different earths, and other matter volatile from extreme minuteness. It seemed, if the other dust could contribute to render substances more fit for producing mould, that it alone did not possess this property. The roots, stalks, and heads of mould, still unripe, were not spared. They were dried, and reduced to fine powder, but without effect. For the most part, instead of the usual quantity of mould appearing, they deprived the sown substances of the power of producing it; and that powder, which did not prevent the production, diminished the quantity of what the unsown substances used to produce. All these united facts seemed to prove, that the granuli, proceeding from ripe mould heads, are real vegetable seeds.

During the course of my experiments, I was curious to learn whether the seed would germinate when sown on substances that naturally did not mould. A quantity was sown on hard bodies, as glass, metals, stones, also on blot sheet, writing paper, cotton, sponge, and the like. All the substances were kept moist: but no traces appeared except some filaments, which were perceptible on sponge. Certain circumstances are requisite for the expansion of the seeds, and these are found only in particular substances.

The

The minute feeds or duft of mould poffefs the peculiarity of refifting a degree of heat, which no other feeds can fupport without lofing the power of germination. After boiling the feed in water, I poured the water, then become black, on fubftances apt to mould, and where it ufually grows thicker than on fubftances unmoiftened. The fame was done with duft expofed to much greater heat; and I have found, that as this heat does not deprive the feeds of the property of reproduction, neither does it alter their fize or figure, which examination with the microfcope both before and after expofure has demonftrated.

But does that mould, which fprings without being fown, and by the care of nature alone, on infinite fubftances, every where difperfed, alfo derive its origin from the duft, which we may fuppofe differninated through the air and on terreftrial matter? If natural and artificial mould are of the fame fpecies, and if the artificial is produced by the duft of the natural mould, I cannot fee why the latter fhould not originate from the fame principle, efpecially fince it is demonftrated that no other part of mould, fuch as roots and ftalks, aid the reproduction. The hypothefis, fuppofing that this duft is invifibly fcattered through all, and gives exiftence to an immenfity of natural mould, is one of the moft reafonable hypothefes in philofophy. If each ripe head can

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furnish a thousand seeds, and if each spot of mould contains a prodigious number of heads, it is clear that in the course of years the dust should be multiplied to excess, because, from its extraordinary levity and fineness, it may be universally diffused.

We have certain evidence that seeds may be kept a long time without losing the germinating faculty. My illustrious friend, M. Bonnet of Geneva, told me a singular fact; In 1748, corn was carried from Sicily to Geneva, and lodged in the magazines of the Republic. Some individuals sowed part of it in a walled garden, 1771. Notwithstanding the length of time, it vegetated perfectly and nearly as thick as common to grain of the same kind. The wonderful minuteness of the seed of mould seems to adapt it for long preservation; but I have already given a convincing proof of the fact.

Heat is undoubtedly one of the most powerful agents in depriving seed of its germinating faculty. In the tract on infusion animalcula, it has been seen that the number of seeds which can support the heat of boiling water is very small; and although M. Duhamel's singular case is cited, where wheat germinated after experiencing 235° in a stove, it is here proved that the seeds of mould are not destroyed by a degree infinitely greater.

greater. Therefore, it is not absurd to suppose that seeds, which resist the injuries of weather, may preserve fecundity for ages. Thus we can easily comprehend how immense the abundance of this vegetable should be, since its seed multiplies so much, is preserved so long, and that it should be so copiously disseminated over all terrestrial substances, as continually to be in readiness for germination, when the requisites essential are present.

The first of Sig. Monti's doubts concerning the production of mould, which led him to think that it originated by spontaneous generation, is resolved. The other doubt which arose from moulding substances acquiring mould after boiling, the same as before it, is equally removed; for if the seed does not lose its germinating faculty from exposure in a hot chafing dish, there is no wonder that it is retained at a degree of heat so far inferior as that of boiling water.

Although the substances for my experiments were constantly kept under receivers, which was done with the view of obtaining more luxuriant and beautiful mould, communication with the external air was uninterrupted. I wished to discover what would succeed on cutting off this communication, but previous to that the consequence of lessening it. Moulding substances were put into very large glass vessels: the necks were then drawn to a point by the blow-pipe; and as the point could

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be brought to any degree of finenefs, I had vef-fels into which a ftream of air, no larger than a hair, could be admitted, a little more into fome, and ftill more into others. All the inclofed fub-ftances moulded in a certain time. But in thofe with a very fmall aperture, vegetation was flow-er; and the mould did not rife fo high as when the aperture was larger. The vegetable fub-ftances within always perfpire fo much, that the vapour collects at the apertures, and obftructs them, efpecially if very fmall, which may be cor-rected by fucking out the moifture; however, if this is neglected, mould will not grow, or hard-ly at all in very fmall veffels.

My curiofity being fatisfied here, I began the other inquiry, which was the effect of excluding the external air entirely, and this was eafily accom-plifhed by a hermetical feal. The veffels were of different fizes, fome might contain fix pounds of water, fome only one, and others but a few ounces. This difference affected the mould. In the largeft, although equally thick as in open veffels, it never grew fo high, and was later of appearing: the mould, in thofe of a middle fize, was fhorter, ftill later, and more rare; its ftate was worft in the fmalleft veffels; none appeared in fome, and in others, a flight fhade was fcarcely vifible.

Veffels of three different fizes, containing ve-getable fubftances, were hermetically fealed, and

put

put ſeveral hours in boiling water. In the ſmalleſt was no mould, a little in the middle ſized, and plenty in the largeſt.

To theſe two experiments I added a third, by putting moulding ſubſtances in vacuo; and repeated reſults proved that, during the time the ſubſtances were there, which was always ſeveral days, if by any chance a portion of air inſinuated itſelf into the receiver, a quantity of mould appeared which came to maturity, though very ſhort. None ever germinated when the air was quite exhauſted. Some plantulæ, produced in a receiver where the vacuum was incomplete, are repreſented, Plate 5. fig. 5.

Theſe three different experiments aſcertain that the plantulæ have the ſame relation with the air as other plants, but it is apparently leſs neceſſary to them; for when a thread of air entered the receivers, ſome mould vegetated, yet the leguminous ſeeds within gave no indications of vegetation; neither do ſeeds vegetate in veſſels hermetically ſealed, although vegetables will mould there. The ſimplicity of mould undoubtedly contributes to render the preſence of air leſs eſſential, in the ſame way as animals, which are leſs compound in the ſcale of organization, may be produced, and exiſt in a ſmaller quantity of air than is neceſſary for us.

M. Bonnet, in his judicious reflections on mould, questions whether we are sure that it all belongs to the vegetable kingdom, or whether there may not be some species that approach the mineral, or, at least, are the link of connection between the two kingdoms, vegetable and mineral. This is not impossible, when we consider the amazing diversity in this class of beings, and how little its species are hitherto known, especially if we consider that fossils approach them by occupying the lowest rank in the order of vegetables.

Besides the mould described, I have not neglected to throw a glance on many different species, and in all I must acknowledge, that characteristics, sufficiently decisive, have been found to judge them real vegetables. But the various kinds observed by me are very few, compared with the immense number yet remaining to be examined; for there is almost no substance, animal or vegetable, which in certain circumstances is not liable to mould. Those who attach themselves to this branch of microscopic botany will have sufficient useful practice, and perhaps may succeed in discovering the link connecting vegetables with minerals, which will render philosophy a most important service. For my own part, I shall be content if, among other things, I have

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have resolved the question concerning the real origin of the most common mould. The subject has not before been completely discussed; and it has led some persons into the ancient and dangerous error of spontaneous generation.

TRACTS

ON

ANIMAL REPRODUCTION,

BY

THE ABBÉ SPALLANZANI

AND

M. BONNET.

RESULT OF EXPERIMENTS

ON THE

REPRODUCTION OF THE HEAD OF THE GARDEN SNAIL,

BY

THE ABBÉ LAZARO SPALLANZANI.

MEMOIR I.

ARTICLE I.—ANATOMICAL DESCRIPTION OF THE HEAD OF THE SNAIL.

THAT the learned and curious reader may the more readily be convinced of this admirable reproduction, it will be most useful to demonstrate, that the parts composing the new regenerated head are not in the least respect different from those of the old one cut off. But we are unable to accomplish this, until with the aid of anatomy, we become acquainted with the parts constituting the head

head of the ſnail, which are much more numerous and complicated than at firſt ſight could be credited. (1).

When one of theſe reptiles (2) extends as far as poſſible from the ſhell, it exhibits the whole neck and head. From the anterior part of the latter proceed the four horns, that is, the two larger above and the two ſmaller below. When completely elongated, each terminates in a globule; but in the larger is a black point, generally thought to be the eye. Immediately below the ſmaller horns, the lips appear; and when they open, while the animal feeds, the teeth are ſeen. All theſe parts, as well as the neck, are covered with minute glandular granuli, ſomewhat ſimilar to thoſe of a ſtrawberry, or like *ſhagreen.* But this ſhagreen on the horns and lips is finer than

(1) This Memoir is extracted from my work on *Animal Reproductions,* which would have been publiſhed before now, if ſome engravings eſſentially neceſſary for underſtanding it had been completed.

(2) The Linnean gentlemen will pardon me for not being diſpoſed to claſs ſnails with worms, as their reſpectable maſter does. This is not the place to ſhew how little that opinion correſponds with nature. It will more properly fall within the limits of my work, when complete, where, among other things, will be a diſcuſſion whether the modern theoriſts and nomenclators have been the moſt uſeful to natural hiſtory.

than on the head and neck. The under part of the ſnail is not ſhagreened; on the contrary, it is very ſmooth and ſlippery. Some naturaliſts call it the foot, and not improperly, from ſupporting the animal in its progreſſion. This is all that the eye can perceive: we are obliged to uſe the ſcalpel for penetrating the interior of the head, which is the object of our reſearch. A ſnail cannot be diſſected alive; however much it is extended, it contracts entirely at the ſlighteſt touch, and, retiring precipitately into its dwelling, lies in concealment there. On breaking the ſhell for examination, the head and horns are found retracted in ſuch a manner within the body, as renders it extremely difficult to make obſervations on them conveniently. The eaſieſt method to follow is that propoſed by the great Swammerdam, in his excellent treatiſe on ſnails, which is killing them ſlowly in water: then they almoſt always remain with the head and neck extended from the ſhell, and the horns protruded. In ſuch an advantageous ſituation, when the ſkin of the head is longitudinally divided with fine ſharp ſciſſars, there immediately appears the brain divided into two lobes; from the under part of which originates the medulla oblongata, and from above, the nerves: four are inſerted in the four horns, and extend to the extremities; the other ſix divaricate to different parts of the body, as the

the muſcles of the ſkin, the mouth, throat, and palate.

As far as I know, Swammerdam was the firſt who obſerved that the brain of the ſnail is moveable, and that its mobility ariſes from ſome muſcles to which it is attached. By means of theſe it is drawn towards the fore part of the head, or is extended, according to the different motions of the body. When the animal ſtretches conſiderably from its ſhell, the brain is commonly ſituated above the œſophagus.

It has already been obſerved, that the black point at the extremity of the horns is commonly thought the eye; and ſuch an opinion ſeems very reaſonable. Indeed the ſingular ability of the often named Dutch naturaliſt, and the aſſiſtance of powerful magnifiers, was able to diſcover the principal parts that characterize the organ of viſion; the uvea, the three humours, and the arachnoid, inveſting the chryſtalline lens. I have ſucceeded in ſufficiently diſtinguiſhing all theſe parts, except the aqueous and vitreous humours, which it has never been in my power to diſcern clearly; but this I rather aſcribe to my inability in examinations ſo minute, than to the non-exiſtence of the parts. Of the four nerves, proceeding from the brain to the horns, two, which we ſhall denominate the *optic*, are attached to the eyes, and enlarge into a kind of gourd or rather

rather pear-ſhaped figure below. Theſe nerves have muſcles, by whoſe action, at the animal's pleaſure, both large and ſmall horns are retracted and concealed in the body; and the eyes, by this means, are alſo concealed, to be ſecure from external injury.

On removing the brain, the œſophagus is diſcovered, which is membranaceous, and furrowed by the fineſt longitudinal ridges of livid aſh colour. The ſides are extremely ſmooth; and it contracts as it gradually approaches the mouth. This opening, by which the ſnail feeds, is provided above with a palate and a callous jaw, to which is firmly fixed a tooth of a corneous ſubſtance, cheſnut coloured and ſhaped like a creſcent, terminated by ſome ſharp prominent points, which form, in a certain manner, ſo many moſt minute teeth, though, properly ſpeaking, the animal has only one; and this is therefore the ſnail's only tooth. In the lower part of the mouth is the tongue, provided with a ſmall kind of corneous ſubſtance at the extremity; and the root is fixed in a hollow ſemicircular cartilage. Theſe are the chief parts of the head, omitting a number of muſcles moving them: and as the deſcription of theſe does not ſeem important, I ſhall neglect it altogether, without committing any fault by the omiſſion.

ARTICLE

ARTICLE II.—REPRODUCTION OF THE HORNS, OR ANTENNÆ.

When I firſt diſcovered that ſnails regenerated the horns and ſevered head, ſeveral philoſophers enquired what motives or reaſons induced me to ſuppoſe theſe parts could be reproduced: and it is not improbable that the ſame queſtion may occur to ſome of my learned readers, to whom I ſhall briefly anſwer, as I have hitherto done. Previous to engaging with the reproductions of ſnails, I was employed with thoſe of earth worms, concerning which there is a chapter in my *Prodromo ſopra le Reproduzioni Animali*, and often had occaſion to obſerve how much the reproduction was promoted, when protected from the injuries of the open air, by remaining covered in moiſt earth or dung. This very ſimple remark recalled to my memory the ſtate of ſnails mutilated in certain parts of the body. I had ſeen them ſhut themſelves up within their portable dwellings, cloſing the mouth with that viſcous ſubſtance which exudes from the body, ſo that the air could with the greateſt difficulty get acceſs. I then reflected, that a mutilated ſnail, retired within its ſhell, and ſhut up with the covering, was in a ſimilar ſituation to decapitated earth worms depoſited in moiſtened earth. This rendered

rendered me desirous to examine whether the same would happen to snails as to worms; and my experiments began with cutting off the horns, which, every one knows, are an appurtenance of the head.

That the section of the horns may be complete, it is material they should be fully protruded from the head, which ensues when the snail stretches considerably from its shell; the whole four then being extended, they may be cut out by the root. If two are divided,—the larger for instance, the smaller are instantly retracted within the head, and the snail partly withdraws into its shell; but, in general, it soon appears, displaying the smaller horns. Hence, the experimentalist may cut off all four one by one, if he chuses. Though the animal is so mutilated, the head and neck are frequently protruded from the shell the same as when untouched.

A drop, and sometimes a little stream of transparent fluid, tending on cerulean colour, proceeds from the part where the scissars divide the horns: this arises from the rupture of the glandular substances. In examining the head, now deprived of horns, we discover four pointed trunks; the points being occasioned by corrugation and contraction of the root of the horn, where it has been cut. If we turn to the separated horns, which adhere to the scissars, we observe that, immediately after divifion, they fwell, becaufe they become confiderably fhorter. The fkin, where divided, either corrugates fo much that the plane of fection is imperceptible, or it enlarges to cover the optic nerve and mufcles moving the horns. The black eye continues vifible at the extremity of the larger horns, after divifion; fometimes, however, it difappears not, but it remains in the feparated horn: it is retracted and buried within, as diffection proves.

The parts of feveral animals, we know, continue to move and live after feparation, for a given time. Such is the cafe with fcolopendræ, earth and water worms, cut in pieces; but particularly, the tails of lizards and water newts, which, for fome time, will move, bend, and leap about, though cut into feveral parts. But quite the reverfe enfues with an immenfe number of animals, fo that the members, whenever feparated from the body, lofe all femblance of life and motion. Snails are akin to thefe. Scarcely are the horns cut off when they become motionlefs, or are only flightly convulfed for a few feconds. No fymptoms of life are afterwards evident, when ftimulated with a point.

If the mutilated fnails are examined in twenty or twenty-five days, it is not uncommon to find the rudiments of a reproducing horn. But this reproduction is very different from that obferved

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in other animals: and here is one of those useful illustrations which teach us to distrust analogical reasoning. The famous Reaumur first shewed, that the principle of reproduction, in the limbs of the fresh water cray fish, began with a little cone in the centre of the trunk, whose base was infinitely smaller than that of the trunk, and, by the process of time alone, became equal to it. A similar phenomenon has been observed by the celebrated Bonnet, in his earth and fresh water worms. The same appearances have been exhibited to me by the tadpoles of frogs, and by water newts, in reproducing the tail and limbs (1). The rays of sea stars, whether casually destroyed by the bite of an animal, or cut off by men, protrude a little cone or tongue from the middle of the trunk, which is the expanding germ of the defective portion. And, in my voyage in the Mediterranean, during summer 1781, I saw several stars, that had lost the rays, budding these cones of different sizes; particularly, the *asterias rubens* of Linnæus; several of which I preserve in the great Museum of Natural History, in the University of Pavia. But the truncated horn of a snail does not advance in this manner. The trunk itself rounds into a little button of a bluish colour, which becomes larger and the colour darker; and at the summit, if we speak of the larger

(1) Prodromo. Citat.

larger horns, is a prominent black point, which is the eye. The reproduced part continues extending, and, in a ſhort time, the new horn equals in ſize its unmutilated fellow. In the ſame manner are the ſmall horns reproduced.

If a half, third, or fourth part is cut off, inſtead of the whole horn, as has hitherto been ſuppoſed, reproduction equally takes place, in the like circumſtances as we have mentioned. This is the more common courſe of nature in reproducing horns: ſometimes, however, the trunk becomes long and pointed, inſtead of round. The point in time enlarges, and forms into a globule; the reſt proceeds on as already related.

The ſnail makes the ſame uſe of the new horns as it did of the old, whether by protruding them from the head, extending, contracting, or concealing them, or by diſplaying their acute and lively ſenſibility; ſo that, on the moſt gentle touch, they are ſuddenly withdrawn and put in ſafety.

All theſe facts ſeemed to aſſure me, that the number of parts, conſtituting the divided part, would be exactly the ſame in the portion regenerated. But, wiſhing to aſcertain the truth by the moſt minute anatomical examination, with a very fine iron inſtrument, I laid open ſeveral reproduced horns; however, I could not diſcover the ſmalleſt difference between the new and the old. The ſame ſkin, externally ſhagreened and within full

full of glandules, appeared; the ſame muſcles moving the horns; the ſame nerves proceeding to their extremities, and there enlarging into an oval bulb; in ſhort, the ſame parts compoſing the eye: therefore it would have been impoſſible to diſtinguiſh the new horns from the old, if I had not witneſſed their origin and increaſe, and if a ſlight contraction of the ſkin had not ſometimes remained at the place from whence they began to ſhoot, or a little projecting eminence which marked the precise ſpot where the new horn had originated.

A ſufficient degree of heat is eſſential to the ſucceſs of this reproduction. Temperate is not enough; and the heat muſt be at leaſt 61°. Therefore, in Lombardy, and different parts of Italy, the experiment ſhould be commenced in the beginning of ſpring. What I ſay of heat, reſpecting reparation of the horns, alſo applies to the head, of which I am about to treat. As the ſummer then approaches, the horns are quickly repaired. With regard to the time requiſite for complete reproduction, two months in general ſuffice for whatever part of the horns is to be regenerated. It may alſo be remarked, that although the reproduction ſeldom fails, I have ſometimes been unable to obtain it, notwithſtanding the mutilated ſnails were kept whole years.

ARTICLE III.—HALF THE HEAD REPRODUCED.

We have ſeen that the conſtituent parts of the ſnail's head are a ſhagreen ſkin, two lips and two mandibles, with a lunar tooth fixed in the upper one: the tongue inſerted into a ſemilunar cartilage, part of an œſophagus; the brain divided into two lobes, and ſending forth ten nerves, beſides four horns of different ſizes. Theſe practical remarks, however, are ſufficient to aſcertain that our experiments, on the decapitation of theſe reptiles, have been correctly executed. If all the various parts formed a head ſimilar to that of moſt inſects, I mean of a globular figure, or were comprehended in a part eaſily diſtinguiſhed from the reſt of the animal, one could at once ſee where it began and ended; conſequently the exact place where the ſciſſars or knife ought to be applied would be known without any hazard of error. But the head of ſnails is otherwiſe conſtructed: when out of the ſhell, the body, if we omit the horns, is a rude figure of a cone, of leſs diameter before, and of greater behind, where confined by the opening of the ſhell. It is a certain fact the head exiſts in the anterior part of the cone, but the difficulty conſiſts in aſcertaining the preciſe portion which it occupies, that we may be ſure how much it is ſafe to cut off. To acknowledge the truth, no abſolute rule can be given, on account

count of the continual extenſion and contraction, the ſwelling and diminiſhing of the cone when the ſnail is in motion. I have commonly found, that the head extends from the obtuſe extremity of the cone to about a line beyond the larger horns, when the ſnail ſtretches to the utmoſt from the ſhell, and there the ſection may be made ſo as ſecurely to take away no more than the head. But if theſe limits be paſſed, ſo as to cut off ſome part of the body along with the head, then we are almoſt certain of the animal's death.

The ſnails on which my experiments have been made with the greateſt ſucceſs are of three ſpecies; the *Helix pomatia*, *nemoralis*, and *lucorum*, to avail myſelf of the appellations of the nomenclator, Linnæus. Several of one ſpecies are deſigned, Plate 6. Fig. 1. The ſnail in its ſhell, but preparing to come out. Fig. 2. It begins to diſplay the head and horns. Fig. 3. The horns fully extended; the ſnail ſeen from below. Fig. 4. The ſnail as far out of the ſhell as poſſible. Fig. 5. A portion of the cone comprehending the head only, which, for better illuſtration, is repreſented ſevered from the neck. Fig. 6. The decapitated ſnail: four points appear on the trunk denoting the ſite of the four horns.

Having obtained reproduction of the horns, it occurred to me to examine what would ensue on severing the head; but apprehensive that the snails would die before repairing the whole, my experiments were begun with truncating a part only, which was that free of the larger horns, and comprehending the lips, the mandible, and tooth, the tongue and the two lesser horns, besides the muscular involucra or integuments. This portion I shall hereafter call the *half cut head*, for the sake of brevity, fig. 4. a. c. That the division may be properly completed, it is requisite for the snail to be fully extended, and the part then cut clean off with scissars, making the section perpendicular to the axis of the cone. But the operation has not uniformly succeeded according to my desire. The head of the snail, as already observed, is extremely sensible, and scarcely is the touch felt, when it suddenly contracts and retires: or, if it is extended, it turns about in various directions. Thence the section often fails of success, if it is made obliquely, and the half head is not exactly severed. But as I judged it of the utmost importance, to the accuracy of the experiment, to learn what parts only were cut off, that it might be known whether the whole would afterwards be reproduced, should such a reproduction actually exist in nature, I imposed a labour on myself, which I had not done

done in my experiments on the reproductions of other animals; and this was an anatomical examination of every half head, immediately on separation. I put the decapitated snails into so many small vessels, each numbered the same as in my journal, along with a brief anatomical description of the parts of each divided head. Thus there was no danger of mistake in my experiments; and I could also learn whether the parts repaired corresponded perfectly in figure and size to the parts cut off. After these remarks, which it is proper to make, we may pass to the narrative of the results.

Immediately after amputation, the snail retires with the greatest precipitation, to conceal itself in the shell; and frequently, in the act of retiring, emits a gentle hissing, which arises from the difficulty which the air finds in escaping from the respiratory canal, that being in some measure contracted by the sudden contraction of the body. Notwithstanding this enormous wound, the snail sometimes soon afterwards comes out, and begins to crawl about in the same manner as when untouched. A snail having lost half the head, and then proceeding from the shell, is designed, Fig. 7. The two points r s mark the situation of the small horns. The large are less than usual, because not fully extended. But the reverse generally happens. However, there is a very easy method

method of forcing the ſnail out for the purpoſe of viewing and examining the wounds, which is, breaking a little of the ſhell behind with repeated gentle ſtrokes of a key or the handle of a knife: the ſnail, irritated by the blows, appears in whatever degree of reproduction the head ſhould be. To avoid uſeleſs repetition, I ſhall here obſerve, that ſuch expedients have prevailed whenever the ſnail refuſed to appear, and gave me an opportunity of ſeeing the cut if lately made, or, indeed, the rudiments and progreſs of the reproducing heads. When the portion is cut off, ſome of that liquid, which in ſnails ſupplies the place of blood, eſcapes: it ſoon ſtems, becauſe the trunk immediately begins to corrugate and diminiſh, ſo as almoſt to diſappear; and in its place is obſerved a ſlight incavation, where no marks of the cut are viſible. The ſnails thus decapitated for the moſt part fix to the veſſels containing them: they conceal themſelves in their habitations, covering the mouth with their whitiſh operculum, produced by the tenacious matter which exudes from the body, and there they remain motionleſs for many weeks, or even for complete months.

When forced to come out, in thirty or forty days, the naked trunks of ſome appear without any marks of reproduction, but others, if the weather has been warm, exhibit a fleſhy globule towards

towards the middle of the trunk, very soft, and of a whitish ash colour, in which there is no organization, either without or within. However, in eight or ten days more, organization is sensible in the globe, then become much larger. The rudiments of the lips are evident, as also those of the small horns, the mouth, and tongue, and a membranaceous dark coloured substance, which, from being fixed in the upper jaw, and cutting through, shews it to be the regenerating tooth of the snail. These parts develope further, and grow more conspicuous; they successively occupy greater space in the trunk; and, in two or three months at most, the divided head is repaired in such a manner, that, unless from the lighter colour, it is not distinguishable from the old one. This, besides external inspection, is demonstrated by anatomy. When the new head is laid open, the same parts are seen, corresponding in number, figure, and size, to those pre-existing in the old, which were scrupulously enumerated in my journal of each decollation. I cannot convey a more sensible idea of this reproduction to my readers, than by comparing it to an unexpanded flower. Considering the rudiments, they are a bud or little globe, consisting of membranes, so involved and aggregated among themselves, that we cannot discover the figure of the leaves or petals, as they may be called. These petals gradually

dually come into view, at firſt obſcurely and confuſedly, then ſo diſtinctly and evident, that every one may recogniſe them as the bud of a flower. In this manner does the involution of the expanding parts become viſible in a real regenerated head.

This complete reproduction is very far from ſucceeding in all ſnails. Two moſt minute globules often proceed from the trunk, in one of which are the rudiments of the ſmaller horns: the other comprehends the rudiments of the lips, the mouth, tooth, and tongue. In proceſs of time, theſe globules are united together, forming one only, and, by further unfolding, conſtitute the half head. It is not unuſual, that one of the two reproduced horns never attains the natural length, or is diſtorted, or that one lip is ſmaller than the other, or even that the new head is quite inclined to one ſide, or a hollow or contraction between the new and the old, or, in ſhort, that the head is not repaired at all; and after ſix months, nay, after a whole year, the naked trunk alone appears, when the ſnail comes forth. When the cut is perpendicular to the axis of the cone, I have almoſt uniformly obſerved, that reproduction has perfect ſucceſs, and that monſtroſity and ſuch anomalous productions frequently happen when the cut is oblique,

and

and the bluntneſs of the ſciſſars prevents the head from being at once divided.

I half decapitated three hundred and twenty two ſnails at different times: the heads of one hundred and twenty ſix were completely repaired. Thirty one had different degrees of monſtroſity or deformity. Fourteen had no reproduction, and the remainder periſhed (1). Thus I have learned, that cutting off only half of the head was fatal to moſt ſnails. All thoſe that repaired the wanting portion, made the ſame uſe of it as of the old, as well in the numerous and ſingular motions peculiar to this part, as in taking the ſuſtaining aliment, ſuch as bread, lettuce, and the like; by which means, from being very much emaciated before reproducing the head, they acquired their original fleſhy fullneſs.

ARTICLE IV.—REPRODUCTION OF THE HEAD.

In the former ſection, we have ſeen that the complete head of the ſnail extended about a line beyond the large horns. In this new courſe of experiments, I determined to ſeparate the head entire, and endeavoured to make the cut exactly in

(1) I diſcovered that half, as well as the whole head, would be reproduced during ſpring and ſummer 1766.

in that place, and perpendicular to the fleshy cone formed by the head and neck. But the same difficulties are found here, as in amputating half the head. Thus the motion of the animals prevented the cut from always being made in the place intended. It has often failed, either from defect by separating less than the whole head, or from excess, by separating more. Therefore, that proper accuracy might be observed at every decapitation, I undertook the same anatomical examinations as before: the snails were preserved in vessels appropriated for the purpose, as also an exact account of the parts they had lost by amputation. Four hundred and twenty three were decapitated.

All those that, along with the head, had lost part of the neck, perished; nor is this surprising, considering that a portion of the organs of generation was likewise taken away. These organs originate within, in one side of the neck, and protrude by an opening when the animal copulates. A number mutilated of the whole head perished; but most of them survived this immense wound, and many completely reproduced the head. But as reproduction was attended by different circumstances, in various individuals, and all worthy of being known, it should be described in a more particular manner.

If a limb is cut off a water newt, the head or tail from an earth worm, the reproduction that

ensues

ensues is an organized whole, that is, a limb, a head, or a tail, in miniature, perfectly similar to the severed one, and only requiring to be farther unfolded. On the contrary, no organized whole, comprehending all the parts of the severed head, appears on the trunk of a decapitated snail, but these parts are frequently separate from the beginning. Thus, some frequently expand after others; and only in a certain space are they all connected together, consolidated, and forming an organic whole, different in little or nothing from the old head. The subject will be more easily understood by examples.

Sometimes the incipient reproduction is a fleshy protuberance, adhering to the middle of the trunk by several points, and in a manner detached from it, which contains the rudiments of the two lips, the smaller horns, the mouth, tongue, and tooth, already repaired. The other parts, such as the larger horns and the rest of the head, are wanting altogether. The trunk of another snail will exhibit a large horn, already provided with its eye, and below, in a distant isolated part, are observed the first lineaments of the lips. In others, the reproduction is a groupe of three horns, two already of their natural size, and the third only a bit of skin. At first, some produce only a protuberance, which, by atten-

tive

tive examination, is discovered to be the lips involved and confined together. Some are already provided with a head complete, all except one or two horns. Lastly, the trunk will exhibit only the two large horns, or the small; or one large and one small.

But all these partial reproductions, and others that afterwards appear, join together in process of time, and by their union form a single reproduction, which is the head, and this, in many snails, is not in the smallest degree different from the old one, except in the lighter colour; by means of which the least observant person can recognise the portion reproduced. One that has completely repaired the head is represented, fig. 8. It has not yet acquired the dark natural colour. Fig. 9. represents the same, only this snail has not yet repaired the two large horns, as sometimes happens. In a little more time, the new head acquires the same hue as the old, and the one can be distinguished from the other, only by an ash coloured line, perpendicular to the axis of the neck, which faithfully indicates the place where the blade has passed in mutilating the snail. This is not constantly a simple line, sometimes it is a deep hollow, almost always of a whitish colour, perpendicular to the neck, if the cut has been perpendicular and oblique, if the cut has been so. In the latter case, the incavation is frequently ly greater where moſt part of the head has been cut off: and, in ſome ſnails, an enormous wound appears on one ſide, though nothing is viſible on the other, or only the aſh-coloured line. And although length of time effaces the incavation, ſtill the indication of the cut, that is, the line, will ſometimes remain two years: nay, even after ſo long an interval, the head is not always complete, for it may want one or more horns; or theſe, at leaſt the whole, have not attained the proper ſize, or are gibbous and monſtrous. Such monſtroſity having frequently occurred, I am inclined to ſuſpect that it originates from the obliquity of the cut, or from being more or leſs advanced on the neck.

The moſt indubitable proof of regeneration ſeemed to be when the heads began to feed. However, I was deſirous of convincing myſelf by the infallible aſſiſtance of anatomy, which has always demonſtrated that the new heads, which externally ſeemed to be completely reproduced, were provided with all the conſtituent parts that I had found in the old heads, which, in each decapitation, had been enumerated to avoid doubts and errors. I may add further, that each new part united, and ſo exactly applied its moſt ſubtile fibres to the old, that we ſhould never have known the ſnails were mutilated, had it not been indicated by the aſh-coloured line ſurrounding the neck.

Here I ſhould not neglect to obſerve, that, in the ſame manner as ſome ſnails, deprived of half the head, never reproduced even in the longeſt time, the like has equally happened to ſeveral of thoſe totally decapitated. Indeed, of four hundred and twenty-three mutilated in this way, thirty-two did not evince the ſmalleſt rudiments of reproduction in a year; ninety-three could not reproduce better: the heads of one hundred and forty-five were regenerated with monſtroſities; and the remainder died. Reproduction of the whole head requires nearly the ſame time as that of the half.

Should I be aſked, why there is no reproduction, either of the head or horns, in different ſnails? I ſhall ingenuouſly confeſs myſelf unable to advance any thing concerning it but ſimple conjectures. As the reproducing and unreproducing ſnails are both of the ſame ſpecies, we cannot ſay that ſome of them have the property and others have it not. I ſhould rather ſuppoſe, that the reproductive virtue cannot take effect from the diſeaſed ſtate of the ſnails; having uniformly obſerved, that, beſides their moſt remarkable emaciation, the exterior aſſumes a yellowiſh hue, which ſeems the inſeparable concomitant of ſnails affected by diſeaſe, and liable to periſh.

After

After obtaining reproduction of the head, it was very natural to think that ſnails would regenerate other parts leſs eſſential; ſuch as, the eminent collar which ſurrounds and ornaments the back of the animal, when out of the ſhell, and the flat broad foot by which it is ſupported during its motion. Theſe two parts, when cut off, were repaired in the beſt poſſible manner; nature reproducing much or little, according as it had been cut away.

Uniting, in one point of view, what we have hitherto ſaid of the reproductions of ſnails, it clearly appears, 1. That they can repair the horns, whether partially or wholly amputated; 2. The head, when half is cut off; 3. That it is as completely reproduced, though the whole has been ſevered; 4. Whatever part of the collar or foot is cut off, it is regenerated. From which it is evident, that theſe reptiles recover preciſely the parts that they have loſt. This is not peculiar to them, for it takes place in other reproducing animals. If a newt wants a third or fourth of the tail, only a third or a fourth is reproduced; and the ſame may be ſaid of one half, or the whole tail. A ſimilar phenomenon ſucceeds in the fore and hind legs of this amphibious animal (1). Reproduction of the parts wanting only, extends invariably to worms, as alſo to frogs, while yet

 tadpoles,

(1) Prodromo citat.

tadpoles. Besides, on consulting the authors that have treated of animal reproductions, I find that the same unalterable laws have been observed; whence we may establish this general rule, if some anomalies are excepted, *In animals, endowed with a reproductive property, nature restores no more than the parts or organs of which they have been deprived.*

ARTICLE V.—FACTS RELATIVE TO THE REPRODUCTION OF THE HEAD OF SNAILS.

THIS Memoir shall be terminated with some new experiments, which I hope will not be disagreeable to the reader, as they will elucidate this interesting subject. Hitherto three species of snails have been mentioned, the *pomatia*, *nemoralis*, and *lucorum*. But are these the only snails enjoying reproductive powers? The experiments of foreign naturalists demonstrate that other species participate the same privilege. Yet I must acknowledge, that my experiments on others have almost always been void of success, for they have either not repaired the head or horns, or only exhibited the first rudiments of reproduction, which terminated with the death of the animal.

But whence does it arise that some species of snails have the faculty of reproduction, and others have not? Should such a question occur, I must candidly confess, that I have had not sufficient

sufficient materials to explain it. Was the organic structure of the reproducing snails very different from those that do not reproduce, some reason might be assigned for it; or, to speak more philosophically, was there any disparity between them. However, no such thing was found, at least by me, with any certainty, among the various species of snails that either succeeded or did not succeed in reproducing.

This uniformity of organization in snails specifically different, which at the same produces opposite results, as reproducing and not reproducing, is a useful lesson that we cannot avail ourselves of analogy in reasoning from one species to another; but that truth may be attained, we are under the necessity of undertaking as many experiments as there are animals specifically different; and the force of this conclusion, so humiliating to us, is more plain and evident, by throwing a transient glance on the various animals which naturalists have at different times discovered to be endowed with a reproductive virtue.

When the immortal Trembley first displayed the prodigies of the polypus to the philosophical world, it was thought that the very simple structure of the animal chiefly contributed to effect them. Indeed, the polypus being destitute of a heart, veins, and arteries, and, consequently, of the real circulation of fluids; neither brain, spinal

nal marrow, or nerves, nor any other concomitant of these parts, which are met with in an infinity of animals, being found in it; but the whole appearing of a gelatinous and homogeneous substance, universally covered with a number of granules; all this, I say, excited belief, that the simplicity of structure concurred in markng the portion cut from the polypus become an entire polypus. Thus it was thought, before the Genevese philosopher's discovery, that the wonderful phenomena, afterwards seen in polypi, succeeded in plants from their very simple structure. And the reproduction of parts lost by other animated beings of simple structure, as sea nettles, sea stars, and I may also add, crayfish, favoured the opinion. But it having afterwards been found, that certain fresh water worms, though much more compound than polypi, when cut in pieces, would become so many complete worms, it was demonstrated, that simplicity of organization was not a condition requisite for the reproduction of wanting parts (1). Besides, this is verified in the clearest manner by earth worms, since Reaumur has found that, cut in pieces, they multiply like plants, which is a physiological fact that some naturalists have denied, but it will be put beyond all question in my *Riproduzioni Animali*. When

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(1) Bonnet Traité d'Insectologie.

I name the earth worm, I ſpeak of an animated being whoſe organization is a thouſand times more complicated than that of the polypus, from finding the circulation of blood, and, conſequently, arterial and venous veſſels in it: an alimentary canal, ſpinal marrow and nerves, and the union of two ſexes, as it is an hermaphrodite. Of the ſame complicated ſtructure is my freſh water boat worm; nevertheleſs its reproductive faculty is not inferior to that of the earth or freſh water worm (2).

Yet how much higher does the organic ſtructure of ſnails and newts place them in the animal ſcale? Let us omit the former, as enough has already been ſaid of them, and ſtop a moment to conſider the latter. Though naturaliſts have properly claſſed the water newt among amphibia, it is a real quadruped, as it poſſeſſes in miniature moſt of the parts which quadrupeds have large. A great number of theſe have the tail provided with oſſeous vertebræ inſinuated into each other, and ſucceſſively ſmaller as the tail diminiſhes. The tail of water newts is of the ſame configuration, and conſiſts of the ſame oſſeous vertebræ: it alſo has the ſoft ſolid parts, ſuch as the *medulla longa*, which traverſes each vertebra, and perforates the ſmalleſt: it has nerves, muſcles, veins, arteries,

(2) Prodromo.

teries, a heart, a numerous aſſemblage of glands containing that acrid milky fluid which exudes through the ſkin when the newt is irritated. Both the fore and hind legs have nearly as many ſoft ſolid parts, and hard ſolid parts or bones, as thoſe of quadrupeds, and almoſt as we have ourſelves. Finally, as a rudely circular bone ſurrounds and terminates the mandibles of our amphibia, from which a little regular foreſt of the ſharpeſt teeth projects, Who could have ſuppoſed that this quadruped had the property of reproducing ſuch an aſſemblage of parts ſo different among themſelves? But it is moſt undoubted: for, beſides being the firſt diſcoverer myſelf, I have witneſſed it repeatedly. Loſing the whole four limbs at a blow is nothing to a newt, becauſe it can reproduce them all, and reproduce them perfectly. I have taken the trouble of numbering the bones in theſe limbs, and found them to be ninety-nine: and ninety-nine have exiſted in the four reproduced limbs when all were amputated from the trunk. Nay, let the four legs be completely cut off, and the whole tail, as alſo the two mandibles; the newt, in addition to reproducing the limbs, will at the ſame time repair the jaws and the tail. This fact, which has ſo much the appearance of a paradox, and at firſt ſight ſeems more fabulous than the famous Lernean Hydra, I have repeatedly ſeen

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and ſhown to many friends, to the great amazement of them all. The water newt is ſo much more the object of admiration, ſince it never defrauds the eager experimentaliſt of its multiplied reproductions, which is otherwiſe with ſnails, as ſome of them will not reproduce.

Behold the evident exiſtence of reproduction, beginning with the polypus, proceeding to various worms, then to ſnails, and, laſtly, to water newts, that is, advancing from the moſt ſimple animals to others leſs ſimple, and from theſe to ſome whoſe organic ſtructure is more complicated; and there is no eſſential difference produced by the more ſimple or more complex organization.

Theſe facts alſo prove, that the tenderneſs or delicacy of fibre is by no means a condition neceſſary for animal reproduction. How great is the difference between the body of a polypus and the tail or limbs of a water newt? But do not both reproduce in the ſame manner? How many ſmall animals are there, as delicate as the polypus, even much more ſo, and as completely aquatic, which, inſtead of reproducing when cut aſunder, inevitably periſh? as I have aſcertained by numerous experiments.

However it is proper to remark, that of the animals adapted by nature for reproduction, thoſe provided with the more tender fibre have a ſin-

gular

gular prerogative over the rest. In the first place, the defective limbs begin sooner to be repaired. A polypus, divided into many pieces, in a few hours begins to multiply into so many other polypi; an earth or water worm requires a few days; a snail or newt, on the contrary, requires several weeks, before beginning to reproduce. Secondly, reproduction is much sooner complete in the former. Only a few days are necessary for a polypus; worms require whole weeks; the snail must have several months for repairing its head; and a year is not sufficient for the new limbs of a newt to grow as large as the old. Thirdly, the same animal, so long as young, and the fibre consequently more tender and pliant, will reproduce the lost parts quicker. This I have seen in newts, and also in snails, which will repair the severed head in six weeks, and much sooner, if young. Finally, reproduction is more tardy, as the natural softness of the animal decreases. We have a striking instance of this in frogs. If, while still tadpoles, but the limbs beginning to appear, their limbs should be amputated, by my own observations it is certain that they will be most completely repaired. But the same will not succeed when the tadpole has assumed the figure of a frog: then it is never, or almost never, that the trunk puts forth a new limb. Whence arises so great a difference in the

the same limbs of this amphibious animal? Shall we say, that this virtue, this reproductive power, which the animal enjoyed while a tadpole, has been lost by it becoming a frog? as if, by the metamorphosis, it ceased to be the same animal, which is but an unphilosophical sentiment (1). I find it more consistent with truth, to suppose the reproductive power continues in the frog; it is enabled to operate in the tadpole, by means of the great tenderness of fibre, but its action is afterwards prevented from the succeeding induration. Let us endeavour to elucidate this a little. The frog, while a tadpole, never leaves the water; and it would perish on attempting to do so. Only from time to time does it dart from the bottom to the surface, and, for a moment, puts up its mouth, to expel the air from its lungs, and inspire what is fresh. The trunks of the amputated limbs then remain in a state of the greatest softness, being always immersed in water, and bathed by it in every point. The minute limb, yet a germ, will be able to perforate the trunk, if this expression may be used, to come out and freely expand. But the same thing will not happen, when the frog has attained its full and permanent size. Then, as it generally remains out of the water, or retreats thither only when menaced by danger, the trunk will be subject

(1) Differtaz. de Fis. Animal. et Vegetab. T. 2.

ject to the influence of the air: thus it will cicatrise; and the contraction, occasioned by the cicatrice, will prevent the reproducing germ from breaking through and expanding. The mutilations of snails, hitherto mentioned, were for the most part performed when spring was somewhat advanced; because I had observed, that no less than 61° of heat was necessary to obtain reproduction, which we generally have not in Lombardy before that time; and if they are then decapitated, it is certain that the snails, at least many of them, will repair the head. But what will ensue, if decollation is towards the middle of September, or when the requisite heat does not continue with us more than a month, on account of the supervening autumnal rains? I have instituted many experiments for elucidating this curious enquiry, and obtained the following results: If the mutilated snails were exposed to the heat of a stove, equal or surpassing the necessary degree, I was sure of reproduction before the end of winter. If kept in a situation where, for some days, they might be exposed to the cold of freezing, the greater part perished. When the cold was less, they retained the power of reproduction, which re-appeared in spring; and the head and horns, having begun to grow at the commencement of winter, attained their full size in the subsequent spring. If decollated in the beginning

ginning of winter, and care taken that they did not perish with cold, though without being kept in a ſtove, no reproductive principle appeared on the plane of the trunk; but it became evident in May, and advanced to perfection during the ſummer months.

The ſame mode of regeneration practiſed by nature in the reproductions of ſnails ſhe alſo practiſes in thoſe of newts, of earth worms, and water worms; with one variation, however, that theſe animals reproduce, though ſlowly, at the temperate degree, which ariſes either from the great ſoftneſs of their fibre, or from ſomething peculiar in their nature.

When engaged with the reproductions of earth worms, it ſtruck me to try whether the reproductive power was exhauſted by the firſt reproduction, and I found that it was not. Thus to the ſecond reproduction ſucceeded a third; and this being taken away, there came a fourth, then a fifth, and ſo on. If a portion of ſuch ſucceſſive reproductions were cut off, the ſecond reproduction entered the firſt, and the third the ſecond, &c. Thus I came to have a ſcale of reproductions united to the old trunk, always younger, ſmaller, and the colour gradually lighter.

Theſe regenerations of reproductions equally ſucceeded in the tail of tadpoles, and, what is more ſurpriſing, in that of newts, and likewiſe in their

their limbs, though the parts of these two members are so different from each other. Therefore, if the four limbs and tail of a newt, already reproduced, are amputated, other four limbs and a tail will regenerate a second time; and this experiment may be considerably protracted. Indeed, with young water newts, where the reproductions were prompt, I obtained six successive reproductions of the limbs and six reproductions of the tail during the months June, July, and August. In one of these animals, I counted six hundred and eighty-seven bones reproduced.

In consequence of these reproductions in earth worms, tadpoles, and newts, I thought of trying whether they would succeed in snails; and, for that purpose, mutilated several, some of the horns, and others of different portions of the head, or the whole; and I cut off whatever parts were renewed, exactly where they joined the old trunk. The second reproduction did not fail to take place and succeed in the same manner as the first; and this also happened with a third reproduction; but the death of the snails prevented me from extending these curious experiments further.

Another suggestion occurred, besides this enquiry, which was to investigate whether the reproductive powers could at last be exhausted, or whether they would always succeed, so long as the

the animal lived. And the experiment might have been begun on newts in preference, selecting the youngest, because this amphibious animal is easier mutilated than snails, more tenacious of life, and reproduces more readily; but I had not leisure for such experiments, which, if undertaken by any expert naturalist, will undoubtedly tend to utility, and may form a new chapter in physiology.

In the whole course of this memoir, I have delivered the results of my experiments only, and always suppressed the details. Had I related these, it would have been a volume, not a memoir; but I was desirous of instructing the reader, without fatiguing him by long circumstantial narratives. I always hope to have obtained sufficient credit with the public to merit belief. It gives me pleasure to observe, that almost the whole of these results are confirmed by distinguished naturalists, as will appear in a second memoir on the reproductions of snails, where an abstract shall be given of the writings in my favour, and, at the same time, one of all those that attempt to controvert them. The confirmers, as far as I know, are Messrs Turgot, Lavoisier, Tenon, Heriffant, Bonnet, Senebier, Schaeffer, Roos, Muller, Scarella, Troilo, besides three other Italians, celebrated professors of anatomy, who, having recently repeated my experiments, and found

found them true, have been ſo obliging as to communicate their memoirs. The controverters are Meſſrs Murray, Wartel, Cotte, Bomare, Adanſon, Schroeter, Argenville, Preſciani. In my ſecond treatiſe, I ſhall likewiſe examine the value of the impugnations with philoſophic impartiality, and ſhall not fail to give the merit of the impugnators its full weight. I am very far from ſuppoſing that my diſcovery will form an epoch in natural philoſophy; it will rather form an epoch in the hiſtory of the human mind, to ſee how an experiment, ſo certain, ſo eaſy as that is, of obtaining the reproduction of the head of the ſnail, has deceived ſuch a number of philoſophers,—and, what is more aſtoniſhing, in an age which ſeems to be that of obſervation and experiment, if, on the other hand, it was not remarkable that experiments are made by every one. But the proper method of experiment has always been, and will always be, confined to very few.

MEMOIR

MEMOIR II.

INTRODUCTION.

I KNOW not whether in our days there has been any natural phenomenon which, from novelty and singularity, has made as great a noise in the physical world, has given birth to so many experiments, and altogether to results so various and opposite as the reproduction of the heads of snails. Since the publication of my *Prodromo*, and the translation of it into the French, German, and English languages, it is incredible how many of these reptiles have been decollated; partly by some whose account of their experiments first imparted to the world that they existed; partly by ordinary naturalists, or those of some celebrity, but little less than unskilled in experiment, and partly by illustrious naturalists who, in the art of experiment, enjoy a distinguished reputation. How was it possible that so great a differ-

 ence

ence among the experimentalists did not produce considerable discrepancies in their results? Thus it has actually happened, that some, very far from obtaining the reproduction of these testaceous animals, have beheld them die, which has given occasion to dispute and reject my discovery, from considering it more imaginary than true. Others, having obtained only the rudiments of reproduction, have not thought it false but exaggerated. However, the rest, by obtaining complete regeneration of the head, have amply confirmed it.

As the principal results of my own experiments were narrated in the first memoir, I have now to relate those of others: explaining with amicable impartiality, as well those favourable to my cause as those that are adverse to it. But it should be observed, that I shall only speak of the experiments both for and against, that have appeared in print, or which unpublished have been communicated to me by distinguished naturalists. Although there are other experiments on the subject, and these neither few, nor made by men of contemptible fame, I do not consider them deserving of repetition, because they have not been acknowledged by their authors, who, surprised at the singularity of the fact, had no object in view, good or bad, but to make them, and were indifferent about committing them to writing, much less to render them public.

To

To elucidate the matter, and preserve regularity, it will be proper to divide the treatise into three articles: The first will enumerate those experiments that controvert my discovery; the second will comprehend those confirming it; and the third will embrace a few reflections that I have esteemed applicable for removing doubt, and placing the truth in a clearer point of view.

ARTICLE I.—EXPERIMENTS ADVERSE TO THE REPRODUCTION OF THE HEAD OF THE SNAIL.

We may call the *Avant Coureur*, a weekly publication in Paris, the field of battle, where many authors have contended both for the reproductions of decapitated snails and against them. The first opposition was made in 1768. 'M. Wartel, 'about the end of October 1767, decapitated 'many snails, which suddenly retired within their 'shells. With great surprise he saw them issue 'from their dwellings full of life, but without the 'head, in May 1768. He does not imagine that 'the reproduction of the head is possible: since 'none of his repaired it, and some did not even 'renew the horns.'

Father Cotté repeated these experiments, as appears from the same journal, May 1769. In June, taking advantage of a shower of rain, he decapitated

tated a number of ſnails with a pair of ſharp ſciſſars. Four of twelve died in eight days, and only one of the reſt ſurvived: it lived more than a year, but without the ſmalleſt appearance of reproduction. In the ſame month, he decapitated other twelve. Some, which he thought had undergone the operation, ſtill exhibited the horns complete; whence he concluded, that the ſnail may ſo ſuddenly retract the head, that only part of the ſkin or integument is cut off, and thus deceives the obſerver. Thoſe, however, leſs alert, and which really loſe the head, he is fully convinced never repair it.

He continued decollating a prodigious number of ſnails during 1770, 1771, 1772, 1773, and with nearly the ſame conſequences. All died in time without reproducing the head. If the ſciſſars ſeparated only the ſkin or horns, theſe parts were repaired.

In the ſame year, 1769, M. Valmont de Bomare publiſhed his experiments in the Berne Journal. 'He repeated Sig. Spallanzani's experiments along with M. Borie. They have obſerved, that decapitated ſnails immediately died, exhaling an intolerable fœtor. Of 52, only 9 were alive in 24 hours; and none except thoſe in which the bluntneſs of the knife had made the cut between the horns and parts of generation, and did not go through. The horns were

'ſenſibly

" sensibly retracted when the skin and upper jaw " were cut off: and the animal, having come out, " displayed the mutilated horns.'

To these three naturalists should be joined M. Argenville and Schroeter. With respect to the former, I have not been able to procure his work: but Mr Murray quotes it in these words: 'Of " an hundred snails, hardly 45 were alive the day ' after decapitation.' The German naturalist decapitated several hundreds: as they all died, he thinks that he is authorised to deny that these reptiles renew the head: and never having had the least part of the horns or tail repaired, he equally denies the possible reproduction of these parts (1).

There are still some authors who do not entirely controvert my discovery; for they admit reproduction of part of the head, but deny that of the whole. These are Messrs Murray, Adanson, and Presciani. Murray mutilated two of the species *pomatia*, so that the head was severed behind the large horns. One died in a week: the other reproduced, though in miniature, for it was only a small horn, and as if in despite of nature: it was shorter and thicker than usual, and wanting the black point at the extremity, which is

R 3 commonly

(1) Vers. e. System. abhandl. uber. d. Erdconchylien. Berlin, 1771.

commonly reputed the eye. He mutilated other ten, in various ways, of the species *nemoralis*. In about six weeks, one, deprived of the horns only, had two reproduced, but both wanting the eye. Those decollated retired into their shells, and could not again be forced to appear. Mr Murray's patience being exhausted, he broke one of the shells, and saw two points like the rudiments of horns already protruded. A fortuitous event prevented the continuance of his observations. From what he had seen, he declared his resolution to preserve a middle course between those who denied the reproduction of the head, and those who admitted it.

Somewhat analogous are the experiments of Dr Presciani of Arezzo in Tuscany, inserted in the *Giornale di Pisa*, 1778. He ends the detail of his experiments thus: 'In my opinion, I have 'been able to ascertain, that the snails, which had 'lost part of the brain along with the head, died 'sooner or later according to the portion taken 'away: that the others, which had lost the 'horns, lips, teeth, throat, and tongue, without 'the brain being touched, lived as long as they 'could survive without food. Those deprived of 'the horns and integuments of the head have all 'had perfect reproduction.' M. Adanson is also in the list of experimentalists, not that he has published any writing specifically on the subject, but

but from a letter to M. Bonnet, we know that he has facrificed more fnails than any other naturalift, as more than 1400 were decapitated in one year. He had partial reproductions even immediately, of horns, heads, lips, &c. but thefe were reproductions of parts not entirely cut off: fince all the heads, horns, jaws, &c. completely feparated, never manifefted the fmalleft reproduction.

ARTICLE II.—AN ACCOUNT OF THE EXPERIMENTS WHICH CORROBORATE THE REPRODUCTION OF THE HEAD.

NOTWITHSTANDING the multiplicity of the experiments related, I have never found myfelf in the leaft inclined to repent having publifhed, in the Prodromo,* my fingular obfervations on fnails, and that becaufe I was too well affured of having feen what is ftated there, and without any apprehenfion that deceitful appearances had impofed upon me. In the end of fpring 1766, I had witneffed the renewal of the head and horns; but I delayed laying this wonderful phenomenon open to the world until 1768. I was not content with having once beheld it; I wifhed to fee it anew, and to return to it often, at the fame time obferving the moft minute anatomical examination, as well of the severed parts of those renewed, to ascertain whether, in all strictness, the part reproduced might truly be called the head of the snail. In my literary correspondence with M. Bonnet, I regularly recounted the whole series of facts, respecting the beginning, progress, and termination of the reproductions, of which he was completely convinced, as appears by many letters to me on the subject, now printed in his works. And of the same opinion were all the learned persons, to whom I had shewn the reproductions, both in Modena, while professor in that university, and here in Pavia. Even about the end of June 1782, having some snails that were mutilated in May, and reproducing favourably, I had the pleasure of shewing them to two respectable friends, excellent judges of such matters, M. Tissot and Father Barletti, who, after examining them strictly in every part, agreed, without hesitation, that the parts repaired were not the *cap* or *bonnet*, as M. Adanson humorously calls it, but true, most certain, most undoubted new heads. Therefore M. Tissot, about the beginning of July, did me the favour to carry some of the reproducing snails to Geneva, to be presented in my name to M. Bonnet, who, on the tenth of August, wrote to me; 'All the snails arrived in the best state; and have 'shewn their admirable reproductions better than

'those

'those on which I made my experiments. Your's 'are of a species with which I was unacquainted; 'they seem in some respect to approach the 'large species of our country, but differing in co- 'lour and the smallness of size. Reproduction 'is evidently more easily accomplished in your's 'than in these, or in the French species, which I 'conclude both from my own experiments and 'those of others.' And he adds, 'Your snails 'have given the best evidence of reproduction, 'by gnawing through the paper that confined 'them.'

From my experiments, I was conscious I had not been deceived, whatever the authors in the preceding section might oppose; considering also, that their experiments were negative, and mine positive. The logic teaching a positive fact is not overturned by a thousand negatives; because the accidents are innumerable, and most of them unforeseen, that may disturb the success of an event. Yet, notwithstanding all these reasons, had my own experiments only, argued in my favour, I should not have been completely satisfied. But, besides M. Bonnet, the rest of my learned friends, who, with their own eyes had beheld this physical fact and with admiration, were perfectly convinced, still I had reason to doubt whether the same would be admitted by an enlightened public; and every author, who publishes his discoveries,

coveries, ſhould be very earneſt that this will be the caſe. Thus, the readers who, after peruſing my experiments, proceeded to thoſe of my opponents, found themſelves between two contradictory authorities; and, notwithſtanding the confidence with which I had made a poſitive aſſertion in the affirmative, this was inſufficient for diſſipating every ſhadow of doubt or ſuſpicion from their minds, which might be excited by the unheard of ſingularity of the fact, and perhaps confirmed and augmented by ſuch a number of other facts tending to impugn it. There was no method more effectual towards conviction, than that this diſputed reproduction ſhould come under the rigorous inveſtigation of able naturaliſts; and that they ſhould aſcertain it in ſuch a manner as to leave no room for contradiction. This has happened. Beſides the internal complacency which every philoſophic enquirer feels in finding his own experiments confirmed, I alſo have the ſatisfaction to obſerve that moſt of theſe naturaliſts have been pleaſed to take my part, without the leaſt relation exiſting between them and me, or the ſmalleſt literary intercourſe. Therefore I judge it of great importance to my cauſe, which is that of truth, to collect together the facts that favour it.

In

In the *Avant Coureur* are also some experiments corroborating mine; the first of which is this.

'H. Roos, a German, now residing in Paris, 'has undertaken to verify Sig. Spallanzani's experiments on decapitated snails. Some died; 'others survived the operation. One reproduced the larger horns; another, which had been 'decollated at the root of the horns, acquired a 'new head and four new horns,' *Journal*, No. 30, 35, 47, 1768.

This experiment corresponds with those of M. Lavoisier, related in the same journal, No. 38. 'We now speak of Sig. Spallanzani's singular 'discovery, concerning the reproduction of the 'head of the snail, with more confidence, because 'M. Lavoisier has shewn the Royal Academy 'of Sciences a snail which he decapitated on the '12 of last June, which has reproduced a new 'head exactly similar to the first. He has several more, with the reproduction less advanced. 'The head was cut off a little beyond the large 'horns. More than a month elapsed before there 'was any symptom of regeneration, which a 'small papilla or tubercle announced. The horns 'are much thicker than in their natural state, 'and only a line and a half long. M. Lavoisier 'has remarked, that the part called the tail is also 'capable of reproduction.'

But

But M. Lavoiſier is not the only academician who has ſeen the reproduction, as we learn from the memoirs of the Royal Academy. 'Since Sig. 'Spallanzani's diſcovery, many ſnails have been 'decapitated. From the obſervations communi-'cated by Meſſrs Turgot, Tenon, Heriſſant, and 'other philoſophers, the head was completely ſe-'vered from ſome ſnails; of others, longitudi-'nally ſplit up, and one half taken away; and 'ſome had the horns cut off, or pulled out.

'In about a month, a protuberance forms in 'the middle of the ſection, which gradually 'grows, and at laſt becomes a new head, pro-'vided with its mouth and teeth. M. Heriſſant 'has demonſtrated that theſe teeth are reproduc-'ed, having ſhewn a ſevered head, with the teeth, 'preſerved in the ſpirit of wine, though there 'were alſo teeth in the regenerated head of the 'ſame ſnail.'

'The horns are not reproduced until the head 'is entirely formed; and their growth preſerves 'no uniform rule.'

Theſe are all the experiments, ſo far as I know, made in Paris. And I have next to ſpeak of thoſe undertaken by other naturaliſts, in different parts of Europe: and, as I am deſirous of continuing the ſame chronological order which I have hitherto endeavoured to preſerve, we ſhall, in the firſt place, advert to a treatiſe by Muller of of Copenhagen, printed 1769, and republished in his work, *Vermium terrestrium et fluviatilium succincta historia*. And as this treatise is not very short, I shall only give an abridgement of it.

1. The German naturalist accomplished decapitation with very fine scissars, when the snail was fully stretched; taking the greatest care not to cut off the part usually called the foot. 2. Immediately after decollation, he constantly examined the amputated head, both with his naked eye and a magnifier, and also shewed it to learned men, who beheld not only the four horns, the extremities of the two larger with eyes, but the mouth, lips, and jaws. 3. Though part of the skin and horns may be taken away, and not the head, an exact observer cannot be deceived, by examining the severed portion which remains on the blade of the scissars; and he easily judges whether or not the operation has been properly performed. 4. The reproduced parts are distinguished from the rest by greater whiteness and transparency; while those separated always continue obscure. 5. Many accidents evitable and inevitable may impede reproduction; but a single operation, accurately performed, will prove the fact without reply.

These are preliminary remarks; and the sum of Muller's observations is as follows.

Both

Both the *pomatia* and *nemoralis* he found inept for reproduction, but it was otherwise with a variety of the latter. On the 19 of July 1768, one was decapitated, and part of the neck, along with the anterior portion of the foot, likewise cut off. It was long before reproduction commenced. The foot, however, was repaired on the 16 of September; but the head did not appear before spring 1769: and in summer, the whole was complete, except the mouth, lips, and smaller horns.

In another snail, decollated 14 September 1768, the upper lip was already formed, March 1769, and the opening of the mouth had become visible. A third, decapitated 14 September, had repaired most part of the head during the following summer.

Muller terminates his memoir with observing, that a subsequent journey had prevented further examination of the reproducing heads; but that the restitution of separated parts had been indisputably proved.

The Padré Scarella of Brescia repeated my experiments in the same year, 1769, and communicated the result in the following letter: 'After 'four months silence, I am now to relate what it 'has been in my power to observe respecting 'snails, and also the observations of a skilful ana- 'tomist, my friend Sig. Ludovico Pusini, who, 'I hope, will in time publish his anatomical and 'physiological

'physiological discoveries. On the 27 of April, 'in presence of many professors of medicine and 'philosophy, I decapitated sixty-two snails a little 'below the large horns. Until the end of last 'month, they remained undisturbed in a box, 'exposed to the air of the window in a close 'apartment; only, from time to time, I threw 'away those which were evidently dead from the 'fœtor. Since the tenth of September, I have 'found twenty-two alive, which protruded the 'head partially, or entirely reproduced when sti-'mulated. And this was clearly recognised by 'every person present. The head of one was 'completely repaired; another had the two 'large horns; others, part both of the large and 'small; and, in all the rest, the progress of re-'production was various. Sig. Pusini shewed 'me a snail with the head completely repaired.

'I am, with great esteem, your most humble 'and devoted servant, GIAMBATTISTA SCA-'RELLA. Brescia, 28 September 1769.'

The year after this gentleman politely communicated his observations, I understood, from my illustrious friend M. Bonnet of Geneva, that M. Schaeffer of Ratisbon had lately published an account of his experiments, which confirmed mine in a surprising manner. Being very earnest to obtain further information, I resolved to address the author himself; from whom I had this

obliging

obliging letter: ‘It has ſo happened, Signor,
‘ my experiments and obſervations on ſnails ful-
‘ ly verify your’s. Perhaps my curioſity has been
‘ extended a little farther: but I regret that the
‘ diſſertations are written in German, and that I
‘ have not had leiſure to make an abridgement
‘ for you in Latin or French. This defect can
‘ only be ſupplied by ſubjoining a brief extract
‘ which a friend has compoſed. And I remain
‘ your moſt humble and obliged ſervant, GIA-
‘ COMO SCHAEFFER. Ratiſbon, 8 March 1770.’

Extract concerning the reproduction of ſnails.

‘ In the year 1753, M. Ziegenbalg, a learn-
‘ ed Dane, preſented a memoir to the academy
‘ of Copenhagen, where he communicated that
‘ ſome ſnails that had been decapitated, were ſtill
‘ alive, and continued to come out and retire in-
‘ to their ſhells as uſual. Although this muſt
‘ have appeared a very extraordinary phenome-
‘ non, it does not ſeem to have met with the atten-
‘ tion it deſerved. Nor was it until March 1768,
‘ that Father Boſcovich announced to M. de la
‘ Condamine, that the Abbé Spallanzani had
‘ decapitated ſeveral ſnails; and not only did they
‘ live, but, after retiring a certain time into their
‘ ſhells, came out as they do naturally, and at laſt
‘ had regenerated a new head organiſed like the
‘ firſt.

‘ Obſervations

'Obſervations of this deſcription could not fail
'to attract the attention of naturaliſts: and the
'celebrated Schæffer of Ratiſbon, in particular,
'has put this phenomenon beyond all diſpute.
'That eminent naturaliſt, having frequently re-
'peated experiments on ſnails, had the head and
'alſo the tail reproduced. Laſt year he publiſh-
'ed his experiments in a tract illuſtrated with co-
'loured figures.

'They have lately been repeated. On the
'ninth of May, twenty-ſix ſnails were deca-
'pitated: Only two died from the operation; all
'the reſt lived, and regenerated the head.'

In the year 1769, Sig. Ab. Troilo, librarian to his Serene Highneſs the Duke of Modena, and Emeritus Profeſſor of Experimental Philoſophy in the ſame univerſity, engaged in the ſubject, and next year communicated his reſults to me.

On the fifth of May, one hundred and twenty-four ſnails were mutilated. The whole head, and a portion of the neck were cut off 68. From 28, the head and no more. From other 28, one half of it, that is, the portion including the two ſmaller horns, the lips, mandibles, tooth, and the various muſcles appertaining to theſe parts.

Forty-nine of the firſt diviſion had periſhed againſt the twenty-ninth: but one began to repair a portion of the head: ſome others had regenerated the left larger horn, and all the reſt, except one, exhibited a principle of reproduction.

More snails died in June; and on the 14, of all the sixty-eight deprived of the head and part of the neck, not one remained alive.

With the 28 decapitated, without injuring the other parts, it was very different. Only one had died twenty-four days after decapitation: and the originating horns of almost the whole were budding; or, indeed, the rudiments of the head; and the regeneration of some was complete in the beginning of June, for they ate lettuce presented to them.

It was much more credible, that reproduction of half the head would be easier obtained. The Abbé terminates his letter, remarking that, notwithstanding the facility of reproduction, the head of some snails was not entirely repaired during the first days of September; and of the monstrosities attending the various generations, one was very singular: it consisted of two horns of unequal length, situated on the right side of the head, which, after being always united, and as if incorporated together, came at length to form a single horn thicker and shorter than a large one is naturally.

Several years after these experiments, and when I was thinking little about them, three new treatises were published confirming my observations,

tions, two by M. Bonnet, and the third by M. Senebier, librarian to the Republic of Geneva. The chief motives inducing both these naturalists to experiment on snails were the doubts started by Messrs Adanson and Murray against reproductions. M. Senebier's account of his experiments is in a letter to the Abbé Rozier, and inserted in the Journal de Physique. I shall transcribe the whole as it is very short.

'I avail myself of this opportunity to announce, that I have repeated the celebrated 'Abbé Spallanzani's singular experiments on the 'regeneration of the head of decollated snails. I 'have lately perused the Gottingen Gazette, 'where there is an extract from a memoir by 'Professor Murray, which puts the matter in 'doubt: and because I know the truth of these 'observations has been questioned by no contemptible observers both in France and Italy, I 'now write you this, as the season is favourable 'for reproduction, and fit for repetition of the 'experiment.

'I decapitated twelve garden snails, 1 5th April, 'and ascertained that separation was complete, by 'inspecting the severed head with a magnifier, 'and comparing it with Swammerdam's exact description. In three weeks, one exhibited the 'head and larger horns reproduced: the small

'once

"ones also budded, and the snail was soon as "perfect as before decapitation."

M. Bonnet, by numerous and correct experiments, established the truth of mine (1).

Here terminates the results by which Messrs Roos, Lavoisier, Turgot, Tenon, Herissant, Scarella, Schæffer, Troilo, Senebier, and Bonnet have confirmed my discovery. But this discovery, I hope, will acquire greater confidence among the learned, from being enriched by three inedited treatises; the publication of which I am certain will be most agreeable to the reader, as they are the production of three able Italian professors of anatomy, already celebrated by their interesting works: and these are, Sig. Caldani, Girardi, and Pratolongo, junior. I lately requested these gentlemen to make experiments on our reptiles, and with philosophic candour to inform me of their success. They complied; and the following letters are transcribed in the order of time that I received them.

"Most esteemed Friend,—Behold me at last "prepared to satisfy your request, by relating the success of my observations on the re-"productive faculty of snails. The simple recital "of facts will be sufficient; for the journal of ob-"servations

(1) I conceive it superfluous to give any extract from his Memoirs, as they immediately follow this.—T.

'servations is too long, and so ill written, that it 'would give you considerable trouble to read 'it: nor have I time to make a correct co-'py.

'My experiments began last July; and I used 'the snails commonly found in gardens, of a 'figure approaching to spherical, and with a 'dark brown shell. Seventeen, some large and 'some small, were decapitated behind the large 'horns. Decollation was performed on a smooth 'wooden tablet, with a strong sharp razor. The 'snails instantly retired into their shells: a viscous 'frothy matter exuded from the trunk, in many 'of a greenish colour. They were immediately 'put into a vessel among soft earth, and covered 'with a sieve.

'Next I proceeded to dissect the heads, after be-'ing kept a little in warm water; and in all, found 'the upper and under part, with the horns, 'mouth, lips, teeth, and a portion of the pha-'rynx. I know not whether I should inform 'you, that these snails were what the French call '*escargot*, and are described by Swammerdam in 'his *Biblia Naturæ*. The general appearance 'seemed to correspond with this; but, assisted 'with a good magnifier, some difference was vi-'sible, as Swammerdam's are in various places 'covered with small isolated tubercles of an irre-'gular figure, whereas mine were wholly cover-

'ed with longish tubercles closely united, and
'like so many elliptical pellucid vesicles, almost
'all of equal size, but not isolated, and they
'were surrounded by another substance evident
'to the eye. When the animal's body was ex-
'tended, the vesicles were long; when contrac-
'ted, they became round.

'From this imperfect description, so able a
'naturalist as you, will recognise the snails which
'were the subject of my experiments. Perhaps
'the French word *escargot* means snails in gene-
'ral, and no particular species.

'Only four of the seventeen snails survived.
'Whether this unlucky circumstance was owing
'to the excessive heat of the weather, or my too
'impatient curiosity, not considering how the
'wound might be affected by the air, I cannot
'well determine. Two died in four days; a
'third on the twenty-eighth, other two on the
'thirty-first day; and three more lived thirty
'three. Then I did not find one dead until the
'sixty-sixth day, and the rest on the eighty-
'fourth.

'All had exhibited some marks of reproduc-
'tion; two had the large horns half regenerated,
'the lips and teeth also made great progress.
'There was a monstrosity in the horns as they
'were united. In the other eleven, a tubercle
'arose from the trunk, and in the centre of it a

'black

'black point, which included the horns joined 'in one. In other five, that repaired the head 'completely, were two black points on the sides 'of the tubercle, and a black line was afterwards 'observed connected with them. These points 'were manifestly the eyes and the lines, the optic nerves. One of the heads was not repaired 'in less than four months and a half. The reproduction was undoubted, because all four, 'which are still alive, fed on young lettuce, and 'also drew the operculum within the shell.

'There is no wonder that nature has provided 'a protection such as the cover; for decapitated 'snails, from long abstinence, want that gluten 'with which their loss is repaired; and it is always thinner, according to the duration of abstinence.

'The reparatory matter of the operculum fails 'in different periods of time. Some snails cease 'to repair it in nineteen days, others in forty-'eight. The defect of gluten alarmed me for 'the safety of my four remaining snails, there-'fore I kept them in a warm situation; and they 'repaired a very thin operculum, which was 'proof they had fed.

'The reparation of the small horns is always 'more tardy, compared to the large ones and the 'lips; neither are the horns always of the same 'length and thickness. I have never seen the 'chagrin be repaired. Yours, CALDANI. Padua, 27th December 1783.'

Next is a letter from Sig. [illegible].

'By your Reverence's moſt obliging letter, I underſtand that you deſire ſome information concerning my obſervations on the reproduction of ſnails. This I conſider an expreſs command, which I the more willingly obey, as I have the pleaſure of numbering myſelf among thoſe who have confirmed your diſcovery.

'My experiments were made in 1780, in conſequence of ſeveral anonymous letters in the journals publiſhed here, where I was defied to ſhow the reproduction of the head of ſnails, decapitated, when I firſt came to this city. Being unable to ſatisfy their curioſity, becauſe all theſe reptiles had periſhed, from the bran, I believe, where I had incautiouſly put them, entirely covering them; and reflecting that it would be to no purpoſe, referring to the ſucceſsful experiments of Muller, Roos, Schaeffer, Bonnet, Lavoiſier, and other moſt accurate naturaliſts, who confirmed this diſcovery, I reſolved to repeat my experiments.

'On the 10 of July, the time reputed moſt favourable for reproduction, I took twelve ſnails of the ſpecies called Pomatia by Linnaeus in his *Fauna Suecica*, and ſimilar to thoſe which I ſent your Reverence laſt year. Anxious that my experiments

'experiments. I have already uſed all neceſ-
'ſary precaution; for, by neglecting it, theſe rep-
'tiles periſh, as muſt have happened to thoſe of
'M. Adanſon, Bonnet, and Father Cotte. But
'I did not follow the plan adopted by others of
'immerſion in water to force them out. Had
'M. Bonnet omitted it, his ſnails would have ſur-
'vived. Perhaps in this I might be too ſcrupu-
'lous. My method was to put them on young
'graſs, and wait patiently till they were ful-
'ly extended, then, with a pair of ſharp ſciſſars, I
'ſevered the head immediately behind the large
'horns. Each ſeparated part was examined both
'by myſelf and thoſe aſſiſting at the operations.
'The ſnails were then ſhut up in a jar, covered
'with a paper full of holes, that the internal air
'might change, and not become prejudicial to
'them.

'On the 28. of Auguſt, I found the paper torn,
'and ſome of the ſnails ſticking to the outſide of
'the veſſel. They all appeared when put on
'graſs, but reproduction had made unequal pro-
'greſs, for only a tubercle appeared on the trunk
'of ſome, while in others reproduction was com-
'plete.

'I ſent many of the ſnails to the editor of the
'journal, with a letter, in which I requeſted him
'to invite our critics to examine my proofs. In
'the next journal, I had the ſatisfaction to ſee an

'anſwer,

'answer, where they acknowledged themselves 'convinced that it was a physical truth, which, 'misled by ill made experiments, they considered 'a prodigy not well ascertained.

'I should not have thought of decapitating 'more of these reptiles, had not the progress of 'reproduction in several, and in the order of va- 'rious parts, seemed strange, and this induced 'me to investigate the principles on which such 'varieties depend. The constancy preserved, as 'I know, from comparing my experiments with 'those of your Reverence, M. Bonnet and Se- 'nebier, convinced me that they were not *lusus* '*naturæ*, but depending on fixed and invariable 'laws. But were these laws regulated by the 'site of the cut, or by its greater or less obliqui- 'ty? This is one of the questions which occurs 'in your Prodromo.

'In the end of last February, I decapitated 'twelve of the same snails as before: but there 'was a considerable difference in the nature of 'the cut. In one half, it was vertical, but a 'little farther from the larger horns than for- 'merly; in the other half of the number, it was 'made more or less oblique, leaving one large 'horn of some untouched. All were then con- 'fined in a vessel. Having examined them in a 'month, I found five dead; however, the other 'seven had formed their operculum. On these, 'therefore,

'therefore, my experiments were to be made:
'but, recollecting Sig. Plateretti's observation,
'that violence done the animals for this purpose
'is fatal, and frequently makes the experiment
'fail, I delayed until July, and then broke the
'operculum; and, having put them on grass, I
'found the whole alive, but the reproduction un-
'equally advanced as in the first. Only two with
'the vertical cut survived; one horn was renewed
'on a whitish globular substance, projecting from
'the trunk of one; and the head of the other
'seemed to be completely regenerated. The
'two large horns were of different lengths. In
'the five cut obliquely, reproduction was a shape-
'less lump, which had not yet acquired its pro-
'per configuration. The horns of some were
'regenerated, and in various states of advance-
'ment; while, in others, they did not even bud.

'As I remarked the same variety in these ex-
'periments that occurred in the first, when the
'sections were similar, I inclined to think the
'inequality of the productive power, acting dif-
'ferently on different individuals, as well as its
'inequality of action on different parts of the
'head, were independent of the cut. But reflect-
'ing that the laws of such phenomena ought not
'to be determined by few, but by numerous
'and repeated observations, I conceived it better
'to

'to suspend my judgment, and to multiply the 'experiments.

'Towards the end of last summer, along with 'Sig. D. Mongiardino, I decapitated a great 'number of snails, diversifying the cut in many 'different ways. It was vertical; one line, or 'farther, behind the large horns; more or less 'oblique; a line, or further, before the large 'horns; and only half the head of others was 'cut off. Ten died, and the rest, examined 'within these some days, were alive. But the 'reproductive power has little activity during this 'cold weather; and it will probably be a long 'time before I am able to satisfy my curiosity.

'With the most profound respect, I am your 'Reverence's most humble and obliged servant, 'GIO. BATTISTA PRATOLONGO. Genoa, 10 'January 1783.'

To Sig. the Abbé Lazaro Spallanzani, royal Professor in the university of Pavia, MICHELE GIRARDI.

'My dear Friend,—I consider your request, to 'repeat the experiments you have made on ter-'restrial snails, as an evidence of that impartial 'anxiety which actuates you for discovering the 'truth amidst the mysterious arcana of nature. 'Thus your observations will always triumph; 'for they are supported on such a basis as to 'dread neither the injury of time nor envy. It

'seems,

'seems, by a fatal destiny, that the luminous and
'useful discoveries of every period have been
'doomed to meet with bitter opponents, who,
'though unable to oppose those beneficent rays
'diffusing around and dispelling darkness, yet
'have sometimes tended to retard the advance-
'ment of science. If I am not mistaken, this ob-
'struction to human knowledge springs from
'two sources; from presumption, which is com-
'monly the child of envy, and from pride.—
'From presumption; because some, who disdain
'observation, know not, and cannot persuade
'themselves that the things which surpass the
'bounds of their limited understanding are true:
'hence, unacquainted with what is wonderful and
'uncommon in the admirable works of nature,
'they deny all which they are ignorant of.
'—From envy, on the other hand, because there
'are certain despiteful men, who will sit on their
'chairs, and, conceiving themselves very learned,
'can ill abide that others know more than them-
'selves: then erecting a tribunal, and, incapable
'of doing any thing else, they imperiously con-
'demn whatever opposes them, or interrupts
'their progress to the summit of that glory, which
'they imagine themselves alone entitled to en-
'joy.

'But among these, I certainly cannot number
'M. Adanson, Wartel, Cotté, and so many
'more,

'more, and, in particular, Mr Adolphus Murray, 'a learned and candid man, and my particular 'friend. Although I do believe, that they con-'troverted your discovery, from not having re-'peated the experiments with accuracy, still it is 'more rational to suppose they have been made 'on snails which want the reproductive faculty, 'as we have often observed. Such are the per-'sons that alone merit an account of this singular 'and admirable reproduction, since it would be 'vain to address others, who neither can nor will 'be persuaded.

'Before transcribing the result of my observa-'tions, permit me to observe, that I have ana-'tomised snails as much as possible, and especi-'ally the head, which is much more compound 'than any one could suppose. But it is extremely 'difficult to accomplish this, while alive, as, who-'ever takes the trouble of examination, will easily 'find. It is equally difficult to make observa-'tions on them in their own proper habitations, 'for they are there so much shrunk and contract-'ed within themselves: and, if the observer en-'deavours to examine them, when stretched out of 'the shell, they contract and retire so quickly, that 'the anterior part of the foot is instantly turned 'out, and the head concealed as if in the centre of 'the body. And the difficulty still augments, if 'we add that tenacious viscous matter copiously

'diffused,

'diffused, and the great muscular power, by 'means of which they contract more forcibly, 'whenever touched by the knife.

'To facilitate every thing as much as possible, 'in my dissections, I practised three methods. 'One proposed by the celebrated Swammerdam, 'to whom we owe so much on the subject, was, 'by allowing the snails to die in water; another, 'by taking away the shell, and thus leaving them 'to perish; and a third, by putting them in cold 'water, and then boiling it a little. By these 'different methods, they frequently remain with 'the neck and head, and sometimes also the 'horns, stretched out, in this manner affording 'me opportunities for convenient examination.

'When a snail extended, and carrying along 'its habitation, is viewed, or when dead, in a si-'milar position, a long neck, terminating in the 'head, appears. Two larger and two smaller 'horns rise from the head. A minute globe is 'at the extremity of each; and in the two larger 'is a black point, called the eye. Around the 'posterior part of the neck is a circular promi-'nence, called the lap, or collar; on the right of 'which appears a hole evidently for respiration; 'and in it the intestine for discharging the fæces 'also terminates. A glandular shagreen skin, 'variously coloured according to the different 'species, covers the upper part of the neck and

'tail;

'tail; and an extremely thin membrane covers 'the under part, with which the snail advances, 'and is called the foot. In the middle are longitudinal lines, and on the sides transverse ones, 'indicating the tendons of the muscles below, 'serving for action and re-action. This anterior 'and extreme part of the foot contracts with the 'lower mandible, leaving a little vacuity, pro'bably that the motions of the mandible may be 'more free and quick. At the root of the small 'horns are the lips; and when these are opened, 'the teeth, tongue, and cavity of the mouth, are 'seen. Under the right large horn, and perhaps 'a little behind and near the foot, is observed a 'small white mark amidst the tubercles of the 'skin, which is the female vagina. And, as all 'snails are hermaphrodites, the male apparatus 'for generation also proceeds from the left side, 'plate 7, fig. 1, b. This mark, which even the 'most practised observers can with difficulty dis'cover, becomes turgid at the season of their 'amours. More than once I have had the good 'fortune to surprise them in copulation, and see 'their mode of proceeding. After they approach 'each other, it is entertaining to behold how 'they mutually examine and re-examine with 'the head and neck, before uniting, and seem 'as if by turns to excite each other and invite to 'copulation. Then, having disposed themselves 'for amorous intercourse, the male apparatus,

'turgid

'turgid with cerulean blood, are protruded from
'the head, and mutually introduced into the fe-
'male aperture, penetrating deep within; and
'they are entwined in ſuch a manner, for two
'hours, or even more ſometimes, that they will
'ſooner ſuffer the rupture of the male organs
'than deſiſt from copulation. By a forced ſe-
'paration, it is eaſy to obſerve how the canals,
'from which the male organs iſſue, ſurround
'and embrace the origin of the appendage to the
'uterus or vagina, and, in that ſituation, how
'much they are diſtended with a cerulean blood.

'All this may be viewed without the aid of
'the ſcalpel; but it becomes neceſſary, when we
'deſire to examine the internal organization, and
'particularly that of the head, which is the chief
'object of our reſearch. On longitudinally cut-
'ting and folding down half the ſkin which co-
'vers the neck, head, and cutaneous muſcle,
'there is, in the firſt place, obſerved, beſides a
'fine arachnoid membrane on the extreme an-
'terior part, a prominent ocular globe, compre-
'hending the two mandibles, the mouth, tongue,
'and alſo the origin of the œſophagus, plate 7,
'fig. 1. b b. The upper mandible is cartilagi-
'nous, and forms the palate within: from the
'higher part a ſemilunar oſſeous tooth projects,
'which in colour and ſhape reſembles one of the
'tortoiſe ſhell combs wore by our ladies for or-

 'nament

'nament on their hair. The tooth is an aggre-'gate of six or seven *dentes incisores*, pointed be-'low, but so united as to form only a single 'tooth. The jaw, or rather gum or under lip, 'has no teeth; it is separate from two soft sub-'stances of a dark colour towards the œsopha-'gus, which confine the tongue. The mouth is 'bounded before and behind by the teeth and 'œsophagus; above and below by the palate 'and tongue.

'The tongue is not free; it is bridled and 'united to the under jaw: two furrowed carti-'laginous membranes form its substance; the 'upper connected with that which covers the 'under jaw, and, curving behind, descends with 'an inflexion, and comes forward under the 'lower membrane; then receding, it terminates 'in a small dark globular appendage, which pro-'jects from the under part of the ovular globe, 'fig. 2. d. This appendage contains a white 'longish curvated mass, which, being moveable 'and connected with the anterior part of the 'tongue, where the œsophagus commences, does 'not seem to be placed there by nature without 'some particular use. The lower membrane of 'the tongue, which is much thicker than the 'upper, convex above, semicircular and hollow 'below; and is rooted in the fore part of the 'under jaw, and terminates free where the upper

'membrane

"membrane is inflected behind. This peculiar
"structure of the tongue seems to supply the defect of teeth; whence mastication is performed by the action and resistance between the palate and tongue.

'Where the mouth and palate terminate behind, the oesophagus commences in the higher and posterior part of the ovular globe, and between the two salival ducts, which unite to the globe by penetrating the cavity of the mouth. It is very narrow at the origin, but dilates further down. It is of a light ash colour, and formed of a thin membrane longitudinally corrugated, by which means it easily dilates and contracts.

'Above the anterior part of the globe is sometimes found the brain, of a palish colour, and in a manner divided into two lobes, fig. 1, i. I say *sometimes*, because the brain of snails is moveable, and changes its place according to the motion of the animal: at times advancing before, and at times receding behind the ovular globe above the oesophagus, especially when the snail stretches out of its shell. This is effected by means of muscles traversing the sides of the brain, and admirably united to it by several connected interwoven filaments. The brain is but about a line in breadth, and not more than one and a half in length; larger, 'however, in proportion to the ſize of the ſnail. 'It comprehends numerous nerves; and al- 'though they have been accurately deſcribed by 'the celebrated Swammerdam, it does not ſeem 'totally unconnected with our inquiry to enu- 'merate them here: obſerving however, if there 'ſhould be any trivial difference, that the fault is 'not to be aſcribed to another, but I will rather 'be content that it is attributed to my inferior 'dexterity in examining objects ſo minute; and 'more particularly, as, in the courſe of this in- 'veſtigation, thinking myſelf able to diſtinguiſh 'the objects without any hazard of error, I have 'uſed no magnifier to increaſe them above their 'natural ſize.

'Of the nerves that divaricate into the head, 'ſome proceed immediately from the brain, others 'by means of a ganglion formed of the lobes of 'the brain itſelf, which they inveſt according to 'its different poſition, ſometimes the anterior 'portion of the ovular globe, and ſometimes the 'œſophagus. They are little larger than a millet 'ſeed below and alſo behind the globe. Fol- 'lowing the order of diſſection, we ſhall firſt 'ſpeak of the nerves of the brain, and then of 'thoſe of the ganglion.

'In the brain originate twelve nerves, that is, 'ſix on each ſide, which I ſhall call ſix pair: 'ſome run behind, and others before. The

'firſt,

'first, which we may denominate the muscular, 'originating in the lobes of the brain, proceed 'back to insert themselves into the muscle *retrac-* '*tor* of the ovular globe; the other five are dis- 'tributed before, and are the upper and under 'labial, the mandibular, the optic larger and 'smaller. The upper labial arise in the anterior 'part of the brain, and, traversing the sides of 'the oval, divide before into two conspicuous 'filaments; one proceeds to the higher part of 'the upper lips, and the other terminates in the 'lower part of them. The mandibular nerves 'originate in the posterior part of the brain, and, 'proceeding back, are inserted in the ovular 'globe, in the vicinity of the salival ducts, di- 'verging into the mouth, throat, and palate. 'The optic nerves issue from the sides of the 'brain, and, by means of either membrane, are 'attached to the muscle *motor* of the larger horns, 'and terminate in a pyriform bulb at their ex- 'tremity. The smaller optics, or more properly 'nerves, of the small horns also arise from the 'sides of the brain, in the vicinity of the larger 'ones. They unite to the muscle of the smaller 'horns; and, divided into several filaments, pro- 'ceed to terminate in the extremity of the horns, 'in the cutaneous muscle and the extremity of 'the under jaw.

'The name *smaller optics*, which I give these 'nerves, may to some appear too arbitrary, and 'perhaps be attended with inconvenience, was 'not adopted from the property of the nerves 'themselves, but from their affinity with those of 'the large horns, which are commonly called 'optic nerves. They had this denomination 'ever since the celebrated Swammerdam, with 'the aid of powerful microscopes, was able to 'find the organs of vision in the black point 'projecting from the extremity, namely, the 'uvea, the three humours, and the chrystal-'line lens invested by its membrane. I will not 'deny this admirable structure, which it would 'not be easy for another to confirm; but my ob-'servations induce me to believe, that if the or-'gan of vision is in these horns, that of touch is 'still more certainly there: and this is the more 'evident, as the organ of touch is most sensible, 'while vision is weak and confused, which gives 'very good reason for uncertainty. I made nu-'merous experiments with a view to remove the 'doubt, but they all concurred in increasing it. 'When the horns were fully stretched, and the 'black points conspicuous, I brought various 'substances close to them, but they gave no in-'dication of certain and distinct vision: because 'I have seldom observed that snails either deviat-'ed from their route, or so much as bent the large

'large horns to avoid the ſubſtances almoſt in 'contact with them. And what further added 'to my doubt was never being able, with the 'bright light of a candle, or concentrated rays of 'a lens, to make the horns alter their direction, 'unleſs when forced to retract by too intenſe 'heat. Although, by approximating a luminous or 'opaque body, they have afforded ſome ſymptom 'of viſion, repeated experiments, which are ad- 'verſe to it, have confirmed my doubts, or at leaſt 'made me ſuſpect, if theſe do ſee, that their ſight 'is exceſſively obſcure and indiſtinct. But as 'their defect is very great in this, ſo are they 'moſt ſenſible in touch. Thus, when naturally 'ſtretched out, the horns are carried well extend- 'ed before them, that they may be warned of the 'ſlighteſt obſtacle, juſt as a blind man is warn- 'ed by his ſtaff to deviate from obſtructions in 'his way. It is ſufficient to behold ſnails advan- 'cing to be convinced of this. They feel or 'grope along; no ſubſtance oppoſed ſtops their 'progreſs, but the horns unexpectedly ſtrike 'againſt it, and then are ſuddenly retracted: 'Next, being extended anew, they ſeem, by 'touching and retouching, to renew the exami- 'nation; and, aſſuming the touch as the rule of 'direction after conſidering the obſtacle, deter- 'mine whether to deviate from their route, if it is 'great, or to continue if it is inconſiderable. If 'the fact be such as it appears, it may reasonably 'be inferred, that snails have no eyes, or, if they 'have, nature has formed them of minute and 'almost unperceptible parts; whence objects are 'seen obscurely and confused: but in compensa- 'tion for the dullness of this sense substituting 'the exquisiteness of touch, by which they can 'distinguish these same objects. Therefore the 'use of the globe on both large and small horns 'may be easily comprehended: namely, that the 'points of contact may increase by the surface 'being enlarged; and the exquisiteness of touch 'thus rendered greater for the use and defence 'of snails. But this is only a simple conjecture 'of mine, started for my own information among 'many others which you will observe.

'Let us return to the organization of the 'brain. We have seen that its lobes compose the 'ganglion, as the annular protuberance is form- 'ed by the lobes of the human brain. Many 'nerves proceed from this ganglion, some for- 'wards, others laterally; some backwards; and, 'lastly, others below. We shall treat only of 'those proceeding forward on the head, as parti- 'cularly belonging to our subject. From the an- 'terior part of the middle proceeds a nerve that 'advances in a straight line to insert itself in the 'posterior and under part of the oval globe. 'From the sides issue two bundles of nerves, one

'from

'from each, which ſpread on the cutaneous 'muſcle in many directions; ſome advancing to 'the root of the large horns; and in the right 'ſide are alſo inſerted in the prepuce and vagi-'na. From numerous ramifications of the lower 'nerves, ſome proceed forward, and are loſt in 'the anterior extremity of the foot.

'In the laſt place are the muſcles of the head, 'conſiſting of the retractor of the oval globe, the 'two retractors of the large horns, and two 'mandibles, which all iſſue from the muſcles 'that proceed from the ſpiral column. The re-'tractor of the globe, which is the largeſt of all, 'proceeds to inſert itſelf in the poſterior part of 'the globe below. Thoſe of the large horns are 'tinged with dark yellow before, and end in a 'ſmall oblong flattened hollow body, that ter-'minates towards the extremity of the large 'horns. The ſmall retractors paſs by the ſides 'of the brain, to which they adhere by means of 'the optic nerves; and thence dividing in two 'towards the extremities, the ſmaller paſſes to the 'extremity of the ſmaller horns, and the larger 'diſperſes in the muſcle of the ſkin below. The 'mandibular nerves, aſcending in ſeveral different 'and diſtinct bundles mutually inclined towards 'each other, proceed to unite at the apex of the 'under mandible, being equally diſtributed in the 'cutaneous muſcle.

'Beſides

'Besides these are others which, although 'small, and resembling filaments, are very ele'gant, and deserve to be distinguished. These, 'which I shall name *fræna*, are seen in the upper 'and under part of the oval globe. The upper 'bridles are simple, Fig. 1. c. but the under 'triple. The former are on the sides of the 'globe, in the vicinity of the salival ducts, and 'terminate in the upper lip. The under fræna 'are divisible into large, smaller, and smallest, or 'long, shorter, and shortest. The long, Fig. 2. 'e e, originate under the ovular globe, where the 'muscular retractor is inserted, and terminate 'within the small horns. The short begin a little 'before the long, about the middle of the globe: 'thence diverging and joining the long, they ad'vance to terminate in the right side of the same 'horns, Fig. 2. f. The smallest and shortest ori'ginate before the short, and terminate in a 'right line in the cutaneous muscle of the un'der jaw, Fig. 2. g. The contraction of these 'bridles draws back the lips, and brings for'ward the globe; which action is so important, 'that it would be difficult to explain how, with'out the bridles, the mandibles could be brought 'so much forward as to be on a level with the 'lips, which is particularly evident when the 'snail feeds, and especially when endeavouring to 'gnaw a bit unsuitably hard.

'After

'Afer speaking of the admirable structure of 'the head, I shall proceed to the result of my 'last experiments on snails, such as they have 'been since 1772 and 1773, with the same con- 'sequences which you was the first to announce 'in your *Prodromo sopra le reproduzioni animali*, 'and have so accurately described. The expe- 'riments I made were on some of the snails cal- 'led by the celebrated Linnæus, *Helices Poma- 'tiæ, Italæ, Lusitaniæ, Zonariæ, Arbusterum*, '*Nemorales Lucorum, Griseæ*, &c. On the first 'of May 1782, I cut off the larger horns of 'twenty-four of those which you gave me to car- 'ry from Reggio, and from an equal number, 'both the large and small. From twenty-four, I 'cut the extremity of the tail, and as many were 'deprived of the head. A slice above two lines 'broad and ten long was also cut from the side 'of the foot of other twenty-four. These various 'mutilations were repeated the same day on the 'like number of the snails of Parma, of the same 'species as the Reggian. The former were dis- 'tinguished by the letter A, and the latter by B; 'and all the rest being also distinguished, they 'were put into vessels. The horns had been cut 'off with sharp scissars, while full stretched, some 'at the root, others through the middle. The 'head, tail, and foot were divided with a sharp 'knife, when well extended from the shell on a

'hard

'hard plane, thus to be secure of the amputa-'tion, and avoid the inconvenience often attend-'ing it, more especially when performed with 'sciffars (1). The head was severed about a line 'and a half behind the large horns.

'I have uniformly remarked a cerulean fluid 'come from the horns, particularly the larger, 'which bedews the dividing blade. This fluid is 'easily observed, if the operation is performed in 'water, and most perceptible if the water be il-'luminated by the bright rays of the sun. Not 'only do we then discover that the liquid is 'greenish, and is the blood flowing in the ves-'sels of these animals, but also the quantity and 'force with which it spouts. I have sometimes 'seen it continue several seconds, then stop, and 'again spring from the same wound in greater 'abundance. When the extremity of the horns 'is cut off, they frequently appear flaccid, faded, 'and fallen down on the lips, and their inability 'to swell and extend, seems to indicate, that 'one is not far from the truth who supposes, 'that to this fluid alone the protrusion and ex-'tension of the horns are owing: because, I 'have found nothing in all the external organi-'zation, by my utmost exertions, I have never 'succeeded

(1) The same precaution has laudibly been observed by Dr Vincenzo Plateretti, as is mentioned in the *Scelta di Opuscoli* di Milano.

'ſucceeded in finding either muſcle, tendon, or 'cartilage, deſtined for that office. The fluid 'iſſues in greater quantity on amputation of the 'head, in leſs, on mutilations of the tail or foot, 'and it is mixed with a viſcous tenacious matter. 'The ſeparated horns exhibit no ſigns of life, or 'they are very ſhort and feeble. It is not ſo with 'the head: for, ſix or ſeven minutes after am'putation, it diſplays unequivocal ſenſibility when 'irritated. Soon after decapitation, the ſnails 'contract and retire into the ſhell, but ſome re'turn in a little. In the place of amputation, now 'much contracted, a ſmall white mark appears; 'then they travel about as vivacious as if the 'head had not been cut off.

'The facility of motion and livelineſs of ſnails 'would perhaps induce one to think the head 'had not been properly ſevered, and this might 'in general be adopted as an indiſputable cer'tainty. But every doubt is diſpelled by the 'moſt ſcrupulous diſſection. In the ſeparated 'head, we behold the horns, jaws, teeth, tongue, 'and its appendages, all the nerves, muſcles, and 'bridles, the extremity of the œſophagus, the 'ſalival ducts, the prepuce, and alſo the vagina. 'Thus there is no queſtion that the complete 'head has been cut off.

'All the ſnails ſoon contract and retire within 'their dwellings. There they form that white

'tenacious

'tenacious operculum which is produced by 'viscous matter that they generate. By this 'they are defended from external injury; but it 'is not more advantageous to them than detri-'mental to one desirous of viewing them for ac-'curate observation. To do so, I have always 'endeavoured, as much as possible, not to aggra-'vate the effects of such an immense wound as 'they have received, by any additional irritation, 'such as breaking the shell behind, and some-'times, if obstinate to appear, pricking them, 'which are not the actions of a masterly hand, 'or of one accustomed to similar experiments; 'and it is actually not uncommon, that those 'stimulated in this manner die. But, to accom-'plish my object, I endeavour to entice them 'out, so that they may appear spontaneously, 'which has always succeeded on putting them in 'warm water, or exposing them to the soft 'showers of spring or summer; then they are na-'turally induced to come into the open air.

'In those deprived of the large horns, or one 'half, I have never observed any thing new for 'ten, fifteen, even twenty days, nay, most com-'monly a month. In the place of amputation, 'a globule arose, which, becoming larger in time, 'exhibited a black point in the centre, exactly 'corresponding to that in the original horns. 'Two and more frequently three months have

'elapsed

'elapsed before both the large and small horns 'were repaired.

'At first, the regenerated horns are of a pale 'white colour, and the skin is much more deli'cate than that of the head. They gradually 'acquire the proper hue, and form a whole so 'equal, that it is very difficult, even impossible, to 'distinguish the snails that have not lost the 'horns. Dissection proves that the renewed 'horns perfectly resemble the first; they lengthen, 'enlarge, and are endowed with all the sensibili'ty of the first.

'In the same manner as the horns, are the tail 'and foot reproduced. I have remarked, that 'they regenerate with an equal continuation of 'the substance, and are sooner completed than 'the horns. These parts are also of pale white, 'which soon disappears, and no traces of ampu'tation remain.

'What is observed in the horns, tail, and 'foot, is seen in the head, but with the following 'difference. In a month, or about that time, a 'globe projects from the centre, sometimes it is 'towards one side, or, instead of a single globe, 'two will appear, one on each side; the produc'tion of them is not always alike. Two, three, 'and often four months have been required for 'reproducing the head from one in the middle, 'and the same from two lateral globes uniting.

'But

'But from one lateral, I have almost uniformly 'seen an irregular production; for a large horn, 'surpassing the natural size, frequently grows 'from the extremity, and still preserves the pro- 'per organization. In my possession, are several 'snails which have lived a year without this irre- 'gular production undergoing any change.

'The renovation of the head corresponds with 'that of the parts already described. At first, 'the skin is smooth and not shagreened; it is al- 'so of a lighter ash colour, and thus distinguish- 'ed from the old skin, so that the new head ap- 'pears wonderfully applied to the original neck. 'Many of my friends beheld these reproductions 'with amazement; and you, along with the 'learned and celebrated Sig. Angelo Mazza, our 'common friend, when you was so kind as to 'visit me last autumn. The difference of colour, 'which continues several months, afterwards 'changes, and there is no distinction between 'the new and old skin, but a faint furrow at the 'place of amputation, which also disappears in 'time.

'Dissection proves the exact resemblance of 'the reproduced to the original heads, because 'all the parts found in the one are found in the 'other; and what is more singular, although 'there is some difference between the colour of 'the neck and the head, all the internal parts are

'so

'so connected, equal and correspondent in co-'lour, symmetry, and consistence, that it is im-'possible to discriminate those reproduced.

'Whenever the head is finished, the snail uses 'the renovated parts to repair the long absti-'nence it has been forced to undergo. During 'the beginning of October last year, I profited 'by a gentle shower to examine all the animals 'that issued from the shell. One of those peo-'ple was with me who are naturally incredulous, 'and never less disposed to believe any thing 'than what borders on the marvellous. Not 'only was he obliged to acknowledge that the 'head was really reproduced, but he observed 'one endeavouring, with its prominent teeth, to 'gnaw a particle of bran, partly adhering to the 'shell, and in an inconvenient place. It was en-'tertaining to see, in the snail's exertions, the re-'peated action and re-action of the open mouth, 'the soft lips, tongue, and teeth, turning the par-'ticle a thousand ways, until it effected separa-'tion, and made itself a savoury mouthful.

'But this admirable reproduction does not 'succeed in all snails, and much less in those cor-'responding to the great snails of Florence, nor 'ordinarily in the small ones called garden snails, 'and especially those found in orchards adorned 'with beautiful colours. My experiments were 'successful on the *helix pomatia*, *itala*, *zonaria*, '*nemoralis*, *lucorum*. Of more than three hun-

 'dred,

'dred, which I have at different times decapitat'ed, ſome are ſtill alive without any regeneration, and ſome with an imperfect and irregular production, and the head of others is completely renewed. I particularly remarked, that, in the greater part that died, decapitation had been improperly performed, or that it had been about four lines behind the large horns; conſequently a portion of the male organs, the appendages to the uterus, or the uterus itſelf, had been cut off beſides the head. Perhaps ſome of thoſe that have not reproduced, or reproduced imperfectly, are of too advanced age, and their fibres have loſt the neceſſary ſoftneſs, flexibility, and vigour; and, by induration, cannot extend to adopt the proper form for perfect reproduction.

'All that I have hitherto remarked happened equally to the ſnails of Reggio as thoſe of Parma, ſo that I have never obſerved any thing deſerving particular conſideration in the latter; which proves that ſnails of the ſame ſpecies univerſally afford the ſame reproductions, if experiments are correct.

'But it is time to terminate this long letter, which has undoubtedly exceeded the limits of your patience. Compriſing the whole, you will only ſee a confirmation of what you cautiouſly advanced in your Prodromo, namely, that ſnails poſſeſs the property of reproducing mutilated

'parts,

'parts, that is, the horns, the foot, tail, and
'head. When observations are accurate, and
'proceed from a philosopher such as you, who,
'in these matters,

"Siete maestro di color che sanno,

'and using the requisite precautions, they should
'easily succeed every where, and most certainly
'do succeed.

'I send you these observations, not from being
'worthy to approach you: I know their insigni-
'ficance, and you also will be sensible of it; but
'because you have desired it, and that you may
'see how earnest I am to give you satisfaction.
'Therefore, be assured that my only anxiety is
'to convince you what esteem and friendship I
'bear towards you.'

FIG. I.

a a The shagreen skin,
b b The ovular globe,
c c The upper fræna,
d The mouth and teeth,
e e The œsophagus and salival ducts at the sides,
f The large horns,
g The muscle,
h The genital organ,
i The brain,
l The left lobe,

FIG. II.

a a The skin,
b b The ovular globe,
c c The muscle retractor,
d The globular appendage of the tongue,
e e The long under fræna,
f The short,
g The shortest,
h The muscle of the large horn,
i The muscle of the small horn,
l The mandibular muscle.

ARTICLE III.—REFLECTIONS.

From the numerous facts in the preceding article, impartial readers will at once perceive that when I first announced, in my Prodromo, the renovation of the head of the snail, I only laid open to the philosophical world an incontestible truth before unknown; and that those authors, who have trusted to controvert my experiments by theirs, have been deceived.

The reproduction which Roos obtained must have been provided with a new brain, a portion of the œsophagus, lips, teeth, jaws, and tongue.

One of M. Lavoisier's snails regenerated a new head exactly resembling the old. The brain must also have been reproduced, because decapitation took place behind the four horns.

No less decisive and judicious were the experiments of M. Turgot, Tenon, and Herissant. It is specifically said, some of the snails were completely deprived of the head.

Muller's experiments wonderfully correspond with those of the Parisian academicians; and when I name Muller, I speak of one of the first German naturalists. The same diligence, sagacity, and circumspection, which, besides the eminent knowledge that characterises his celebrated works,

Plate 7. Vol. 2. Page 308.

works, are alſo diſtinguiſhed in his judicious memoir on ſnails.

Father Scarella, a learned naturaliſt and mathematician, whoſe death Breſcia has ſtill to lament, had the head of a ſnail, decapitated behind the large horns, ſo completely renewed, that, except in colour, it could not be diſtinguiſhed from the old one.

Schaeffer, whoſe name alone is a ſplendid eulogium, does not mention the exact place where the cut was made; therefore it is neceſſary to conſult his diſſertations in German, which I have not ſeen; but he expreſsly declares, that his experiments and obſervations confirmed mine in the moſt ample manner. He alſo obſerves, that he has perhaps extended his curioſity a little further than me, probably alluding to the reparation of the foot. However I had alſo witneſſed the reproduction of that part which ſome call the tail, that is, the poſterior extremity of the foot; and I have even obtained reparation of the whole foot, or the part on which the animal reſts in its progreſſion. This is ſaid in the Prodromo, p. 70, and repeated in the firſt memoir. But I rejoice to ſee the ſame reproduction confirmed by a naturaliſt no leſs expert, ſuch as M. Lavoiſier.

The ſnails deprived of part of the neck, by the Abbé Troilo, periſhed; but thoſe that had loſt the head, and no more, ſurvived, and repaired it

 completely.

completely. One most convincing proof was, that the snails began to feed.

The same evidence was given by one of the snails decollated by M. Senebier; an illustrious Genevan, who has published an excellent treatise on *The Art of Observation*. His precepts have been enhanced by example; for he has enriched philosophy with the best experimental inquiries, and lately with *Memoirs on the influence of solar light on the three kingdoms of Nature*. There could be no doubt of complete decapitation, for he dissected the severed heads.

It cannot be supposed that the experiments of his countryman were less convincing, that is, of Bonnet, whose works constitute the delight and admiration of the age. How many precautions did he not adopt, to be beyond the shafts of his adversaries! What precision did he not use to ascertain that the head was severed! What assiduity, care, and diligence, in minutely observing the phenomena of the reproducing head! With what evidence and impartiality does he not demonstrate reproduction! But these important facts will be highly enjoyed by those who consult the memoirs of that philosopher: for I am well aware that any sketch or extract of mine would be but a faint and languid perhelion.

I should still have to speak of the three letters from Sig. Caldani, Pratolongo, and Girardi, did

I not

I not reflect that the learned will better comprehend the import, on perusing the letters themselves. Sig. Caldani observes, that only four of his decollated snails were alive 27 September 1782. He afterwards wrote to me, 19 April 1783: 'Of the four remaining snails, three died 'during the late cold. The fourth, which was 'the largest of the whole, has for four days shewn 'me a very fine head.'

Sig. Girardi observes, that the moment the horns, particularly the larger, are divided, a stream of cerulean fluid escapes. Both he and I explain this phenomenon, but we disagree in the explanation. In my memoir, I have supposed the fluid first generated in the glandular parts of the horns: he, on the contrary, thinks it is the blood of the animal, or a fluid analogous, that spouts from the divided horn. On better examination of the fact, it appears that he is right and I am wrong. Within the horns, there is truly a large vessel, which even extends over part of the head, full of that light cerulean fluid; and, when the horns are cut asunder, it becomes flaccid and almost disappears, by discharging the fluid whose colour renders it visible.

Passing through Parma last November, I had the pleasure of embracing my most esteemed friend in his own house. He shewed me several snails mutilated during the preceding spring, and the ſeparation had gone far beyond the head. I was anxious to learn the conſequence, which he was pleaſed to communicate in the following paragraph.

'I decapitated the ſnails that you ſaw on the 11 'of May laſt year, fully four lines behind the 'large horns. In the ſevered head were not only the muſcles, and all the nerves connected to 'the oval globe, the ſalival ducts, brain, &c. but 'behind them the anterior portion of the male 'organs, and more than half the appendages of 'the uterus, including the angular part of the 'ovary, ſo that I thought it impoſſible that theſe 'reptiles could reproduce the mutilations. After 'keeping one, in which the cut had even gone 'further, a long time in my hand, and immerging 'it long in water, within theſe few days it appeared ſtretching out its neck. The rudiments 'of the large horns were evident at the extremity, 'and below them the lips and mouth. Almoſt 'diſcrediting my ſenſes, I was ſtrongly tempted 'to diſſect the ſnail to ſee the internal reproduction; but the external parts being incomplete, 'I reſolved to defer my examination. I have 'ſince kept it ſhut up in the ſame box where you 'ſaw it, that I may diſcover in future what perfection the external parts will attain. Parma, '13 January 1784.'

It

It must be observed, that a certain degree of heat is required for reproduction, and not less than 61°. Reproduction is finally nothing but a new generation, with this single inconsiderable distinction, that in ordinary generation an organised whole originates and unfolds, while in reproduction only a part of that whole is developed. The same conditions requisite for the origin of the whole are required for the origin of a part, and among these conditions is heat. In man, quadrupeds, and most other warm blooded animals, the foetus originates at any season, because it is matured in the body of the mother, where there is always a certain degree of heat. Birds may hatch their eggs in winter by means of their heat. It is otherwise with cold animals: they do not and cannot propagate except in warm weather; for during the rest of the year they are in such a situation from cold, that, to judge by external appearances, we should rather say they were dead than alive. Thus it is with most insects, worms and reptiles, among which last, snails ought to be numbered. They neither copulate nor generate except in summer. Shut up in their calcareous dwellings, and the mouth covered with the operculum formed of that viscous gluten exuding from the body, they remain motionless, and in a lethargic state, under the earth all winter. Not only are they incapable of generating aphides, gall insects, wheel animals, libellulæ, artificial fecundation, and so many more which rather seem possible than true.

How far have those departed from real philosophy, who have called the reproduction in question, before endeavouring to verify it! Prejudices, whether favourable or unfavourable for any system, theory, or hypothesis, are in general fatal to observation. When we interrogate nature, it must be divested of all prejudice and passion, which obscures the fair face of truth; and, with an amiable indifference, we ought to judge equally against others as ourselves. If, on the contrary, we are prepossessed with wishes, distrust, and doubts, we shall behold experiments from the side favouring our desire, and not from that adverse to it. Our opinions will be incorrect; and, instead of adding useful facts to philosophy, we shall increase the number of errors.

EXPERI-

EXPERIMENTS

ON THE

REPRODUCTION OF THE HEAD

OF THE

TERRESTRIAL SNAIL,

BY

CHARLES BONNET.

MEMOIR I.

THE only object of publishing my experiments on the reproduction of the head of the snail, is to afford an additional confirmation of the Abbé Spallanzani's beautiful discovery. How much it has been disputed out of Italy, and particularly in France, is well known. There are naturalists of of my acquaintance, who, after decapitating hundreds of ſnails without ſucceſs, have conceived themſelves entitled to conclude, that the Italian obſerver had allowed fallacious appearances to impoſe upon him. One, in a letter to myſelf, did not heſitate to reproach me for inſerting an account of the imaginary diſcovery in the *Palingeneſie*, and for reaſoning on it as a fact completely aſcertained. It will juſtly be thought, that theſe reproaches did not in the leaſt impair that confidence with which the ability and ſound reaſoning of the Reggian naturaliſt had inſpired me. Beſides, he had communicated a full account of his intereſting experiments in a courſe of correſpondence, and it was eaſy for me, only by a trial of the facts, which the learned obſerver had ſeen and ſeen again, to judge of the prodigies he laid before me, and which he ſoon preſented to the public in an Italian Tract 1768, which in the ſame year was tranſlated into French. But the author having there neglected to detail the precautions he had adopted, to ſecure his diſcovery againſt all diſpute, I requeſted him to publiſh an account of his method, which he tranſmitted in a letter from Modena, 11 September 1769, and it was printed in the *Avant Coureur* 30 October. This letter, though perfectly calculated to remove all heſitation, has had but a partial effect; ſome doubts ſtill remain, and people

ple continue oppoſing to the Pavian experiments, thoſe which they ſuppoſe contradictory or apparently confuting them. I have been induced myſelf to repeat the learned Profeſſor's inveſtigations, owing to this conflict of experiments, which has continued nine years. From the account which I proceed to give, the impartial public will judge of the confidence that it deſerves.

The ſpecies of ſnail on which I operated, is of a middle ſize, and frequent in the fields or in gardens after a rainy day; numbers then abandon their dark retreats, and in a ſhort time one may collect hundreds. The ſhell of ſome is yellow, or yellowiſh; on that of others are circular black or brown faſciæ.

It is by no means an eaſy matter to decapitate a ſnail, for the moment it feels the inſtrument, it ſuddenly retires into the ſhell. Thus it is evident, that we may ſuppoſe a ſnail is decapitated, when only a portion of the integuments is cut off. To avoid deception, I have uſed ſeveral precautions. The ſnail is allowed to extend as much as poſſible, and an additional extenſion is procured by immerſing the animal in water. The inſtrument is frequently preſented before ſtriking the blow; and I never eſteem the operation complete unleſs the head is obtained entire, with the four horns fully diſplayed, and alſo the mouth, which may always be recogniſed by the opening

of

of the lips. The head, as it appears some time after separation from the trunk, is represented a little magnified, fig. 1. plate 8. The two large horns, gg, are somewhat contracted; the small, pp, are entirely shrunk within themselves. The mouth b, is closely shut, and the lips are very evident there.

A sharp edged knife seems more proper for this operation than a scalpel; scissars are still less convenient than the latter. I have uniformly observed to make the cut perpendicular to the axis of the trunk.

Immediately after decapitation, the snail retreats far within its shell, and in general does not appear again. A copious portion of that viscous fluid, with which it is so amply provided, is now diffused. Some motion is perceptible in the horns of the severed head, and chiefly in the larger; but this very soon ceases; and I have in vain tried to renew it by stimulating the head near the origin, with a scalpel. All the horns contract to a certain degree instantly on the operation, and the small contract more than the others.

By a very simple method, one may ascertain whether the operation is complete, namely, by immersing the decapitated snail in water. It quickly leaves the shell, and extends as much as before decollation. Then we immediately see whether

whether the trunk is entirely deprived of the head (1). The anterior part of ſuch a trunk, drawn from life, is repreſented, fig. 2, and the profile, fig. 3. plate 8. It is evident that the fleſh is powerfully contracted to cloſe the enormous wound.

The viſcous matter, exuding after decapitation, forms a thin whitiſh operculum, which completely obſtructs the mouth of the ſhell. Two of theſe opercula are frequently formed, one ſituated above the other, ſometimes there are three; the exterior of which is near the edge of the opening, and the inmoſt more or leſs within the ſhell.

Though the decapitated ſnail can reproduce ſeveral opercula, the viſcous fluid is gradually exhauſted, and the ſhell at laſt remains open, or nearly ſo, becauſe the animal, being incapable of feeding while deprived of the head, cannot repair the continual loſs of this kind of varniſh. It inſenſibly becomes emaciated, which is evident

(1) It may happen that the ſnail does not extend ſo much as would be deſirable, or as is neceſſary for judging of the progreſs of reproduction, but it is only requiſite to take the ſhell between the fingers, after removing it from the water, and the animal will ſoon extend as much as poſſible. Great care muſt be obſerved to avoid touching the ſnail, becauſe the moſt gentle motion makes it retire into its ſhell.

by the diminution of ſize and an internal tranſparency. I have often been aſtoniſhed at the number of ſucceſſive opercula produced by decapitated ſnails; however, all do not produce them; at the ſame time, the ſhells of very few continue open.

My decapitated ſnails were kept in boxes. Some remained at the bottom, others attained the ſides, againſt which they applied the mouth of the ſhell; others, aſcending higher, reached the covering, where they fixed in the ſame manner. Theſe ſeemed to be the moſt vigorous, or had ſuffered leaſt from the operation.

When I wiſhed to learn the ſtate of the decapitated ſnails from week to week, I had only to take the opercula carefully from the mouth of the ſhells, and then immerſe them in very limpid water; thus the ſnails are forced to appear ſooner or later, but they have ſometimes remained within for ſeveral hours after immerſion. This method, to which I have always had recourſe, is the beſt in my opinion, for the ſnails, then extending to the utmoſt limits, expoſe the anterior part ſo completely, that nothing can eſcape the obſerver's eye. They endeavour to leave the water, and gradually attain their purpoſe, if the depth is not too great; or, crawling ſlowly over the bottom and ſides of the veſſel, they advance until they reach a dry poſition, and

fix

fix themſelves there; and it is neceſſary, for forcing them out, to re-immerge them in the water. Notwithſtanding privation of the head, they advance forward as if they had one, only their progreſs is a little ſlower.

At firſt, I decapitated only a dozen of ſnails, which was on the 8 of May 1777. I repeat, and it cannot be too often repeated, for I am earneſt to obviate the moſt trivial objections, that decapitation has never been eſteemed complete but when I had the head entire, and accompanied by all its appendages, on my tablet. The heads ſevered in this manner were ranged together along one ſide of my tablet, where they ſtill remain.

Let us proceed, in the next place, to deſcribe the wonderful reproductions which ſucceeded before my eyes. I will not enter into minute detail; it is unneceſſary, for my only purpoſe is to prove the reality of the reproduction, in confutation of the detractors of the famous diſcovery of my friend the Abbé Spallanzani.

This reproduction does not preſerve the ſame uniformity as that of the head of thoſe aquatic worms, which I multiplied by ſections in the year 1741, and of which I publiſhed an account a few years afterwards. The reproductions of the ſnail preſent a number of varieties, which it would be tedious to deſcribe. Signor Spallanzani

zani has given some instances in his *Prodromo*, and after him, I have mentioned them in *la Palingenesie*, part 9; the reader is referred to these two works, for I should here confine myself to my own observations.

A profile of the anterior part of a snail decapitated 8 May, and delineated 21 June, is a little magnified, fig. 4. The two large horns, *g*, *g*, are beginning to protrude; the left is further advanced than the right, which is just originating. A brown, or blackish line proceeds from the large right horn; this is the optic nerve and its muscle; the various motions and structure of which, Swammerdam has displayed to our admiration. Some transparency is perceptible in the flesh; and, as it increases greatly in snails that have abstained a month or two from food, the optic nerve and muscle become much more evident. A white line *l*, runs along the back; but I am ignorant whether it is a vessel. The anterior part of the same snail, viewed from before, is represented from the life, fig. 5: only the upper extremity of the large horns is seen, and at the extremity there appears a minute black point; this is the eye of the snail, in which Swammerdam assures us he has found the three humours of an eye, the two tunics, the uvea, and arachnoid. Here the eye is already visible; though the horn is but in its origin, it is perceptible in those

those that have even made less progress, as will immediately be observed. The small horns have no eye at the extremity; they do not yet appear, neither are the new lips of the mouth *b* visible. This snail I shall distinguish by the letter A.

The anterior part of another snail, drawn 23 June, is represented a little magnified, fig. 6. Reproduction is somewhat advanced, for one of the small horns, *p*, seems completely regenerated, though its fellow has not begun to protrude. The origin of the large horns, which have made very little progress, is seen above the small horn *g*, *g*. This is a striking example of the varieties which occur in the reproduction of the head of the snail; one of the small horns is far advanced, while the corresponding horn is still imperceptible, and the larger ones only beginning to expand. Fig. 7 is a profile of the snail. The transparency allows the optic nerve *t* to be seen proceeding from the origin of a large horn; the eye is distinct, and the lips of the new mouth *b* are visible. This snail I shall distinguish by the letter B. The anterior part, as on the 2 of July, is represented a little magnified, fig. 8, and the section, fig. 9. The mouth *b* cannot be mistaken, nor the large horns with their eyes, *g*, *g*.

Another snail, which on the 23 of June seemed to have completely repaired the head, shall be

 designed

designed by C. The four horns were perfect, and had acquired the natural ſize of thoſe proper to this ſpecies. The mouth ſeemed to be regenerated: the opening was complete; and the new lips, very diſtinct, were of the figure and proportions which they ought to be. In a word, this ſnail ſo much reſembled other ſnails of the ſame ſpecies, which had not been mutilated, that I could diſtinguiſh it only by the tranſparence and diminution of ſize. It is repreſented fig. 10. as drawn from the life: and the plane of the anterior part, fig. 11. where the new mouth and its lips are diſtinctly ſeen. Above it, the tranſparence of the fleſh ſhews an oblong ſpot *t*, which is the teeth of the ſnail: the lips can approach to it or recede. Theſe two figures were not deſigned till towards the middle of ſummer. From the 23 of June, I began to ſupply the ſnail with young vine and lettuce leaves: but it would not touch them. After traverſing the leaves and the ſides of the veſſels, it commonly fixed to the covering, and remained there during complete weeks. Notwithſtanding its abſtinence more than two months in ſummer, it always ſeemed in good health, and is ſtill in the ſame ſtate while I write this, 21 July.

I have already obſerved, that the eyes appear, although the large horns are but beginning to be repaired. This was evident in a ſnail decapitated ed 8 May, whose head is represented, fig. 1. On the 6 of July, when it was designed, the reproduction had made very little progress, fig. 12. Though the origin of the large horns is visible, they do not yet begin to extend: indeed, the eye alone, which is already perceptible, indicates their place: it appears like a minute black point, such as it is possible to make with the finest pen. The snail was drawn when stretched to the utmost, and I have used the same precaution with all the rest. In that of which I now speak, neither the small horns nor mouth yet appear.

When the large horns are retracted, the black point or eye is easily perceived within through the flesh. I have frequently discerned it with my naked eye, and even in those snails whose reproduction was very little advanced.

I should not neglect to observe, that of the twelve snails decapitated 8 May, only one died. All the rest still seem to be well, 27 July: but the progress of reproduction is very various. In some, it is no more than begun: in others, the large horns alone are repaired, the origin of the small ones is imperceptible, and the mouth is ill defined. Some of the large horns are only a half or two-thirds of a line in length. Such are those of the snail, which has hitherto been distinguished by the letter A: the anterior part, as it appeared 21

 June,

June, is represented fig. 4. and as it was 26 July, fig. 13. Something singular is presented by the large horns: they are thicker in proportion to their length than those of unmutilated snails. At the extremity, we remark a kind of deformity, which seems produced by a certain plication of the flesh, and gives the horns a monstrous appearance. However, the eye is very distinct. The colour of the horns tends to violet, which is generally the case with all reproducing horns, and very much resembles that of the nerve traversing the flesh. The mouth is not perceptible in the anterior part of another snail, unless as a prominence arising, *b*, fig. 14.

I have commonly remarked, as well as Sig. Spallanzani, several conspicuous irregularities in the reproduction of the double parts of the same snail. Sometimes a large horn only half or two thirds of the length of the corresponding horn appears, or the latter is scarcely visible. Likewise a small horn will be completely regenerated, while its fellow is hardly perceptible or not at all. I have seen one half of a lip reproduced, and the other fully repaired.

I may limit myself to these few examples; they will be sufficient to give an idea of the varieties presented by the regenerating head of the snail. It seems to me, that they infer the reproduction of one part to be independent of that of another:

another: for how can we deny it, if one horn is completely reproduced, while the reſt are inviſible, or only beginning to grow? This muſt be conſidered a moſt important fact in the theory of theſe admirable reproductions: but I ſhall omit ſpeaking of it here, as I have already endeavoured to ſketch it, Part 10 of the *Palingeneſie.*

On the 12 of May, I decapitated thirty ſnails of the ſame ſpecies, treating them preciſely ſimilar to the firſt: above two-thirds have periſhed. Thoſe ſtill alive regenerate variouſly, and preſenting the ſame varieties, or ſome analogous to thoſe already deſcribed. The months of May and June, and the beginning of July, have been very raw and wet. At ſun-riſe, Reaumur's thermometer ſtood at 4, 5, 6° above freezing on ſome days in the firſt week of July (1).

At preſent, this diſcuſſion ſhall be no further extended, for I intend to reſume the ſubject in another Memoir: but enough has been ſaid to prove that nothing is more certain than the wonderful reproduction. I know not what to think of the fruitleſs attempts of ſome philoſophers, and particularly thoſe of Meſſrs Adanſon, Cotte, and Bomare. Perhaps they have too ſoon declared the ſtate of their experiments, or taken for

(1) About 41°, 43°, and 45° of Fahrenheit's thermometer.—T.

for equivocal what was a real reproduction: or perhaps they thought the snails still alive were dead. In this case, it is requisite to have much patience, and, above all, to despair of nothing. I do not speak of the diversities which the difference of species might occasion in the result of the experiments made by these celebrated persons; for there is reason to suppose, that among the immense number of snails on which they have operated, some were of the same species as mine. Neither do I speak of the diversities that might arise from the difference of climate; for that of Paris is very little different from ours. Therefore I entreat these able naturalists not to be discouraged, and to resume a subject so pregnant with new facts, and one which cannot be too deeply investigated. They possess far more information, talents, and ability, than are necessary to succeed in experiments of this nature: and I may predict the most complete success if they will not be discouraged, and if they will proceed in the manner I have done.

M. Adanson wrote to me concerning his own experiments, 30 July 1769. 'I begin to have a 'philosophic doubt concerning the regeneration 'of the head, horns, and jaws of snails. My ex'periments, diversified to infinity for above a 'year, on fourteen or fifteen hundred snails of 'different species, convince me that my doubt has 'foundation.

'foundation. I have, as every one has had, re-'productions, even very immediate ones, of 'horns, heads, lips, and other parts; but these 'were reproductions of parts that had not been 'entirely cut off: for all the heads, I say, the 'real heads, all the horns, all the jaws, and the 'other parts which have been completely cut 'away, and only a quarter of a line from the 'origin, never exhibited any kind of reproduc-'tion, far less a complete regeneration. Let us 'be strict, and investigate the truth. All who 'have mutilated snails, and first Sig. Spallanzani, 'have certainly been deceived. They have thought 'the head was severed, when the cap only has 'been cut off: they have believed that they se-'parated or eradicated the horns and jaws, while 'the origin always remained; whence it is not 'wonderful if reproductions ensued. These, you 'will candidly admit, are not reproductions, or 'rather regenerations, such as you, M. Trembley, 'and Reaumur, had seen in fresh water worms, 'the polypus, the claws of lobsters.—How many 'well credited operations have deceived persons, 'less familiar than us with similar operations and 'the anatomy of shelled animals! They have 'thought that they had completely cut off so many 'heads, horns, and mouths, beyond the origin, 'which, in every journal and periodical paper, 'they have so liberally regenerated. I am well

'aware

'aware of our deficiency in most nice experi-'ments; and, notwithstanding my great expe-'rience, I may almost presume to say, dexterity 'in the anatomy of the smallest animals, I always 'distrust myself. For this reason I have repeated 'the same experiments an hundred and an hun-'dred times, before hazarding the results before 'the public. I have laboured the first, or among 'the first, to corroborate all the experiments of 'Sig. Spallanzani, and to make additions to 'what might have escaped his notice. I have 'operated on a greater number of animals, and 'diversified my experiments more than any other 'person, to judge of all that has been read be-'fore the academy, or printed; and I am the 'only one who has read nothing on the subject, 'which I investigate with the utmost assiduity.— 'It is nearly the same with the reparation of the 'parts of newts, several species of frogs, toads, 'tadpoles, &c. When part of the tails and feet 'were amputated, I have seen sensible reproduc-'tions; but none when these parts were am-'putated close to the origin. Consider my ex-'pressions well, *regeneration* and *origin*, on which 'your principles rest so much, and there is no 'actual reproduction. And I hope you will do 'that justice to my doubts, as to acknowledge 'that Sig. Spallanzani and his followers have 'too far extended their expressions of regenera-

'tions,

'tions, which were only partial reproductions of 'parts.'

To the numerous experiments and doubts of my eminent correspondent, I shall oppose only the letter to me from the Abbé Spallanzani, cited in the beginning of this treatise, wherein he details all his precautions to avoid error. I sent M. Adanson a copy of this letter, but it did not produce that effect on his mind which I expected; nay, he still persisted in his doubts when he wrote to me, 20 July 1775. 'The various 'parts amputated, or torn, not only from different species of snails, but also from several other 'aquatic animals, as frogs, toads, newts, have 'produced no organized reproduction to me, as 'the mutilated part did to Sig. Spallanzani. I 'have diversified the experiments, which my 'friend Mr Needham and some other observers 'of this rank have witnessed, to such a degree, 'that we all esteem it certain, when the operation 'has been complete, the reproduction is but a 'stump, that is, a mass of flesh unorganized or 'differently organized. And Signor Spallanzani 'should know that the observations of our most 'celebrated anatomists have proved, that the reproduction of the tails of lizards, which are so 'common, although externally well formed, 'present no regular ossification as the rest; nor 'have they any internal vertebræ.'

M. Adanson

M. Adanson is evidently one of those philosophers who start difficulties about facts; and who desire themselves to behold prodigies again and again, before admitting them for truth. This reserve cannot be censured; but I confess that here it appears extreme, especially after evidence so strictly demonstrative as the Abbé Spallanzani has given of his discovery. May I therefore hope, that the experiments, which I now publish, will triumph over the incredulity of our learned academician? Doubtless he will not suspect that I have deprived the snails only of the *cap*, to use his own expression; for the head so completely and so perfectly separated from the trunk, as represented fig. 1, will not allow the least suspicion to remain. I must request M. Adanson to consider all the details of my experiments; and to attend to the able designs of the artist, which admirably represent the regenerations I have witnessed. They might easily have been further extended, but I do not esteem it necessary to the purpose I have in view. Should it be objected, that the snail drawn from the life, fig. 10, 11, had not then touched the young vine and lettuce leaves with which it was supplied; I may answer, that it has given the most indisputable evidence of being provided with very good teeth; for, on the 27 of July, it began to eat the paper covering the mouth of the vessel where it was

confined

confined, and voided some well formed excrements, the colour and consistence of which being exactly like paper, indicated that they were its remains.

M. Adanson also doubted the reproduction of the members of the newt, which has been so fully ascertained by the numerous experiments of Signor Spallanzani, and the principal results published in his interesting Prospectus, 1768. In the letter of 20 July 1775, M. Adanson observes, 'Whenever the operation on newts has been 'complete, only the stump of a reproduction ap'peared, that is, a mass of flesh unorganized or 'differently organized;' and he cites the testimony of Mr Needham, and some other observers. But what will M. Adanson himself say, when I inform him that this pretended stump, or imaginary lump of inorganic flesh, is the member itself perfectly formed, concealed under this deceitful appearance; and which has been completely developed before me, as I had formerly seen the evolution of the head and tail of those aquatic worms, which were multiplied by being cut in pieces. In my cabinet, there are actually newts completely repaired, of which I shall publish a history in a future memoir, accompanied by excellent designs. Our celebrated academician has therefore been precipitate in his opinion, when he thought he only suspended it. He has decided

decided that the newt reproduced but a ſtump; while the ſtump was the member itſelf, where nothing eſſential was defective, and it had juſt to acquire the ſize of that which it replaced.

Thus M. Adanſon was as much deceived concerning newts as concerning ſnails; and the miſtakes of ſuch a naturaliſt are a good leſſon to thoſe who have neither his knowledge nor his ability. I am convinced that he will acknowledge his error, for I know him to be a ſincere friend to truth; and ſtand in no fear of reproach for having laid it open in this little treatiſe.

M. de Bomare, no leſs a friend to truth, and whoſe experiments have been as unſucceſsful as thoſe of M. Adanſon, was conſequently equally incredulous. I had referred him to the ſame letter from the Italian obſerver, printed in the *Avant Coureur*, 30 October 1769; and, on the 5 of November 1775, he replied—'You aſk me why 'I have not anſwered one of the articles of a for'mer letter reſpecting the reproduction of the 'head of the ſnail. I can anſwer you, that all 'the experiments which I have attempted on this 'ſubject ſeem adverſe to thoſe of Sig. Spallanza'ni. You will ſee, at the article Limaçon of my 'Dictionary, what I have ſaid on the ſubject, and 'which I mentioned before in 1768.' I ſhall transcribe from the *Dictionnaire d'Histoire Naturelle* the passage to which M. de Bomare refers.

'I confess that, being unable to credit the re-'production, I made many experiments on the 'subject while at the Chateau de Chantilly dur-'ing Autumn 1768, which have since been com-'municated to the public; and the result fol-'lows. I decapitated fifty-two terrestrial snails: 'all, whenever they felt the sharp-edge of the 'knife, suddenly and very powerfully contracted 'themselves, and the operation finished, the part 'that retired precipitately into the shell appeared 'corrugated like the extremity of the rectum of 'a hen. Nine were twenty-four hours in mo-'tion, and only those which had an imperfect cut 'on the neck between the large horns and the 'organs of generation; the knife being so blunt 'that I had evidently seen all the horns retract 'into the interior of the animal. Thus I had 'only separated the skin and jaw of the snails; 'so that in ten or twelve days, they proceeded 'from the shells crawling about with mutilated 'horns. The snails, which lost the diagonal half 'of the head, displayed no more than two horns: 'but all those that I had completely decapitated, 'which were by far the most numerous, died in 'a few days, except two, which lived five months 'fixed to a wall, and died in spring without in-'dicating reproduction of the head. I have

‘ made a longitudinal inciſion between the four
‘ horns in the head of other ſnails: nature em-
‘ ployed above a month in uniting the parts, and
‘ the animals were very languid. Theſe experi-
‘ ments were repeated, 1769, and all unſucceſsful-
‘ ly. Many perſons have informed me, from dif-
‘ ferent countries, that their eſſays have been ex-
‘ actly ſimilar to mine.’

It is ſingular that I have ſucceeded with only a dozen of ſnails, while M. de Bomare has failed with more than fifty, and M. Adanſon with above fourteen hundred. But it is poſſible that theſe gentlemen were too earneſt to believe that their experiments had failed; or they did not pay ſufficient attention to the progreſs of the regeneration, always more or leſs tardy, more or leſs diſguiſed, and, conſequently, more or leſs difficult to be recogniſed.

I have named another valuable naturaliſt, F. Cotte, curate of Montmorency, who has been equally unfortunate as M. Adanſon and Bomare. His unſucceſsful experiments are related in a letter to the Abbé Rozier, which was publiſhed in the *Journal de Phyſique*, May 1774. The reproductions of heads are according to him *imaginary*. Becauſe he decapitated a great number of ſnails from 1768 until 1774, and almoſt the whole died ſoon after the operation, which had been performed with a ſharp knife, not by drawing but

by

by a single blow; and his account is concluded with remarking three consequences, which he assures us were attendant on all his experiments and observations. 1. He remarks that snails have the property of contracting very suddenly to protect their heads from the instrument, so as to escape with losing only part of the horns, or at most the skin of the head. 2. When it happens that the head is actually cut off, it is not reproduced: at least he declares that he has never seen reproductions, not even of portions of the horns, amputated. 3. Snails can live very long without eating and without the head.

I am ignorant whether M. Adanson, de Bomare, and Cotte have continued their experiments, or what has ensued. But I am not the only naturalist who has succeeded in corroborating Signor Spallanzani's discovery: that has already been done by the celebrated Signora Bassi of Bologna, by M. Lavoisier and Schæffer; and M. Senebier pastor and librarian of our Republic, who has given public testimony of his skill in philosophy and natural history, has had the same success as myself in experiments on snails. I would here transcribe what he has communicated concerning them, did he not say that his observations are sent to be published in the Abbé Rozier's *Journal*.

Although the head of the snail is a very complicated little machine from its structure, as I have shown in another place (1); it is indubitable that the gelatinous quality of the flesh greatly promotes its wonderful reproduction. When treating of the polypus, I have enlarged on this observation. However, I do not mean that one should conclude that all gelatinous animals, all animals in their primitive state of jelly, may reproduce or repair the loss of their members, as the polypus and snail. Experiment alone can discover the limits of this admirable property; and what we have already learned concerning the extent of its dominion should excite naturalists to diversify to the utmost their essays on a subject so fertile in wonders. I cannot exhort them too earnestly to despair of nothing even in the most uncommon experiments.

(1) *Palingenesie Philosophique*, Part 9, new edition of *La Contemplation de la Nature*, Part 3, chap. 25, Note 4.

MEMOIR II.

HAVING myself beheld some of the prodigies presented by the regenerating head of the snail, I wished to know what M. Adanson thought of the result of my experiments, and I requested him to read the preceding treatise, which was published in the *Journal de Physique* 1777. He did so, and wrote me on the 10 of January 1775.

'I have read your excellent Memoir on the 'reproduction of the head of snails, with all the 'attention it deserves; and the perusal only tends 'to confirm me more and more, in what a course 'of experiments, continued nearly ten years, 'since 1768 until the present day, on several 'thousand snails, has taught me, namely, that 'when the integral parts of snails, whether the 'head entire, the eye or the oculated horn, the 'upper jaw are radically extirpated, or cut be-'low the origin, they are not reproduced, either 'under the same form, or with the same organi-'zation as before. You have decollated two or 'three dozen of snails, 8 and 12 May 1777. I 'believe it. You severed the heads, as I myself 'did, two lines beyond the origin; that is, to-'wards the opening of the parts of generation.

'You

'You have taken from each severed head, as I 'have done, the upper jaw entire, and the ocu-'lated horn also entire; then after two or three 'months, in June and July, you have seen a 'third of the snails, even eleven of the twelve 'first decapitated, reproduce a complete head, 'with the eye horns, and the upper jaw formed 'like a horse-shoe, with the serrated teeth. You 'must permit me still to retain my philosophic 'doubt concerning the three last assertions, 'until repetition of the following experiments 'on which it is founded; experiments which 'have served to confirm the accuracy of my 'operations, and certain proofs that my snails 'were completely, and not apparently, decapitat-'ed. To obtain the same certainty, take the 'greatest number of snails you please (that is, 'hundreds, to provide for the great mortality 'that will ensue) not of the small species, called 'the lacquey, which you made use of, and is 'most deceiving, from its great lubricity and 'agility in evading the knife, an agility propor-'tioned to its smallness, and must have misled 'you; at least, that happened to me in my first 'attempts, and obliged me to abandon it; take, 'I say, the large yellowish snail of the vine, nam-'ed pomatia, or rather the brown garden snail, 'by us called the gardener, which is almost as 'large, and the most common of all. After keep-

'ing

'ing the whole a day or two, more or less im-
'mersed in water, and under a press, in order to
'diminish their vivacity and lubricity, tear away
'the upper jaw, which is formed like a horse-
'shoe, and bounded by five or six teeth; the
'lower palate, which is a membrane dentated
'like the tongue of a cat or a file, and eradicate
'the two large oculated horns, using for these
'last little pincers, with thread or flax, to take
'off any edge, and prevent them from slip-
'ping, or, indeed, pressing the neck of the ani-
'mal with two fingers, to eradicate the jaws;
'avail yourself of this forced position to ampu-
'tate with a fine and sharp botanical scalpel, the
'two oculated horns, with the bulb below the
'eyes; or you may separate only one of the
'horns, on purpose to leave an object for com-
'parison. Cut the head entirely off others, ob-
'serving whether the severed heads have the
'jaws and eyes complete. Reserve the whole
'jaws and eyes, that you may be certain you
'have as many as snails operated upon. The
'snails, thus deprived of teeth, eyes, or heads,
'will generally live six months, even one or two
'years, without food; they gradually become
'emaciated even unto perfect extinction; if, dur-
'ing this time, they recover new eyes, new jaws,
'a new head, which I have never had the good
'fortune to see in those identified after the ope-

 'ration;

'ration; if this experiment, made with all the 'precautions I have taken, and which I believe 'it essential to use, succeeds in your hands, and 'with the Abbé Spallanzani, I shall esteem it a 'fact, that the parts entirely separated are repro-'duced in these animals. But, beware, would you 'desire to make such contradictory experiments, 'though without them you cannot be certain of 'the real reproduction of a jaw, an eye, or a 'head. I abridge the subject, because the con-'sequences that may be deduced are in my let-'ter 30 July 1769, which I thank you for hav-'ing reminded me of in your treatises.

'Let us proceed to newts. I have yet been 'unable to peruse your Memoir; but, excepting 'the tail, to judge by my observations on that 'of lizards, it seems incapable of reproduc-'ing the osseous vertebræ; and although I 'have only had reproductions of stumps of the 'feet cut off several species of these animals and 'of frogs, from not being able to prosecute 'my experiments with such conveniency, or 'so long on them as on snails, I most firm-'ly believe the possibility of the reproduction of 'the toes and their bones, when the anterior or 'posterior part of the arm is not amputated.'

I want words to express all the astonishment which this letter from my learned correspondent excited; and I doubt not that the reader will be equally

equally surprised. What M. Adanson desires me to do is precisely that which would occasion the failure of the experiment; for, how could one tear out, or eradicate with pincers, the different parts of a snail, without causing the greatest internal disorder? How is it possible, in this way, to succeed in eradicating all the parts? and, supposing that it was practicable, should we not endanger the sources of reparation? Is it not enough, that I am certain, by the most attentive examination of the heads I have cut from my snails, that they contain all the parts which characterise a head, such as the four horns, the mouth, the jaws, &c.? Was it necessary to cut out the large horns with a botanic scalpel, in order to ascertain that the snail would produce new ones? Was it not sufficient, that I had once and again beheld the origin and progress of the new oculated horns, that I had seen the new eye and the optic nerve first appear in this wonderful reproduction? It is improper to magnify, as M. Adanson has done, the alacrity with which the snail retracts its head the moment it is touched by the instrument; for that alacrity is not so great as to prevent a person, with a little address, from effecting complete decapitation. I can even affirm with truth, that it very seldom failed, when the precautions mentioned in the former Memoir were taken.

M. Adanson seems to reproach me with using snails of too small a size; he remarks, that the small species which were used must have deceived me, from their great alacrity in eluding the edge of the knife. Nevertheless, I can affirm, that I have operated on the small species as easily as on the middle sized, and even the largest. But we exaggerate the alertness of snails, in saving their heads; for, abstinence during several days, and immersion in water, undoubtedly weaken them, and, to a certain degree, diminish the celerity of their motions. Besides, if small snails were mutilated, it was only because I reasonably presumed, that the wonderful reproduction which I wished to behold would be accomplished more easily, or in shorter time, than in the largest snails. However, experiments on the largest species have not been omitted; and the success shall be related.

To terminate this answer to the objections of our celebrated Pyrrhonist, I shall here subjoin an extract from a letter that I wrote to him 21 January 1778.

'If it was in the compass of my power, my 'dear and illustrious friend, I should make the 'experiments you desire on snails. But, in truth, 'I do not think that any thing can be done more 'strictly demonstrative than what has been so 'well executed by my friend Signor Spallanzani; 'and

'and what he narrated at length, in the letter of
'11 September 1769, that I transmitted to you,
'but to which you have never replied a single
'word.

'What is required? To ascertain whether the
'head is completely cut off; and whether the
'head reproduced contains all the organs of the
'original head. What have we to do, that these
'two facts may be proved? The severed head
'must be carefully dissected, and the interior ex-
'amined with the utmost attention, that we may
'be satisfied it contains all the organs belonging
'to it; and it is necessary to dissect the repro-
'duced head with equal care, and, by an accu-
'rate examination of the interior, to ascertain
'whether it actually contains every part pertain-
'ing to the head of a snail. This has frequently
'been done by Signor Spallanzani; and now
'let me ask, whether, in strict logic, there is any
'foundation for doubting an experiment made
'with similar precaution? Yet you tell me, 30
'July 1769, that the Abbé Spallanzani must be
'deceived; that he supposed the whole head was
'severed, when only the cap was cut off. You
'persist in the same assertion, 30 July 1775.
'Certainly you cannot have attended to the
'Reggian observer's letter, to which I referred
'you; therefore allow me to refer you to it
'again.

'I have

'I have set apart all the severed heads, and 'considered the whole attentively. I have re-'marked the two large horns with their eyes, 'the small horns, the mouth, the lips, and the 'other parts: I have then seen new horns pro-'trude: I have seen the eyes of the horns, and 'the optic nerve of the eyes: I have seen a new 'mouth, new lips, and new teeth appear in the 'snails—in the same snails whose original heads 'I had set apart. I have seen snails that gnawed 'the covering of a vessel with their new teeth, 'and void excrements containing the matter 'which they had consumed. What more could 'you desire, my worthy friend? And, after so 'many proofs, how can you inform me that you 'still retain your philosophic doubt? Can such 'a doubt, extended so far, and calling in ques-'tion the most accurate, reiterated, and demon-'strative experiments, be termed *truly philosophi-'cal*? Consider that the Abbé Spallanzani 'and I are not the only observers who have 'beheld the prodigies visible in the reproduc-'tion of the head of the snail. The celebrat-'ed Signora Bassi, Messrs Lavoisier, Schaef-'fer, Muller, and others, have also seen and 'described them. Are you willing to think, all 'these observers have been imposed upon;—these 'who have given such ample evidence of their 'ability and accuracy?

'With

'With respect to the reproductions of newts, 'you tell me that you firmly credit the renewal 'of the fingers and their bones, so long as the 'anterior or posterior part of the arm is not cut off. 'I regret that you have written this, before per-'using my treatise on the *Reproductions of Newts*. 'There you would have seen that I amputated 'the fingers, the hands, the cubit, whole arms, 'legs, feet, and thighs; and that all these mem-'bers were perfectly regenerated by the animals. 'This would have induced you to put more con-'fidence in the beautiful discoveries of the Abbé 'Spallanzani on snails and newts. My treatise 'was printed in Rosier's *Journal*, last Novem-'ber; and I wonder that you, who reside in the 'same place where it is published, have not pro-'cured it. The figures, added to the memoir, 'are very accurate; but the designs were supe-'rior to the engravings. It will present you 'with facts, which, I hope, in a little time, you 'will not oppose. See, therefore, and believe.'

The letter from Signor Spallanzani on the mode of operation, to which M. Adanson had paid less attention than it deserved, is so well adapted to convince naturalists of the reality of the ingenious discovery which it describes, that I cannot avoid transcribing it here, as the best refutation that may be opposed to the detractors of this discovery, and as a model of the method that

that should be pursued in researches of a similar nature.

Modena, 11 September 1769.

'I thank you, Monsieur, for your information concerning snails. On considering the different results of naturalists, and particularly of French naturalists, I am of opinion, that, besides their inexpertness in the art of experiment, the diversity of the species of snails, on which they have endeavoured to repeat my experiments, has, in an eminent degree, occasioned a difference in the result of their experiments. It is certain that all the snails of Modena reproduce more or less; but I do not warrant the reproduction of foreign snails: perhaps some among them do not possess this resource. You will see more on the subject, in my preface to the Italian translation of your *Contemplation de la Nature*, which will appear this year. It is very probable that the snails, which have exercised the industry of the learned anonymous Frenchman, of whom you speak, are in the number of those where the reproductive property does not reside in the highest degree. I may say the same of the snails on which M. de Bomare and Father Cotte have operated. But does it thence follow that I am deceived? To affirm it would be a rash assertion, to call it no more. If any

'one

'one attempts to confute me, I will try to defend myself; and my very circumstantial details, as well as those of my friends, will prove that I am in no error.

'You obligingly inquire, whether the severed head truly contained all the organs pertaining to the head of a snail? To answer this important question, I shall mention the mode of performing the experiment. When I discovered that snails enjoyed the prerogative of reproducing their parts, I began to dissect them, purposely to acquire a perfect knowledge of their anatomy. I wished to make myself master of all the organs composing the head. My model has uniformly been M. Lyonet's anatomical work; and I was provided with the whole of his apparatus. The snail, which I proposed to dissect, was killed in water; it then proceeds from the shell; the four horns are displayed, and it dies in this position, which is the most favourable for dissection. It is by this trivial experiment that I have been able to convince myself, that the severed head actually contained all the parts Swammerdam has described in his treatise on snails.

'It was only after having studied the structure of the head attentively, that I began to mutilate the animal; and I proceeded in the following manner: The snail was allowed to extend fully

'from

'from the ſhell, and diſplay the horns; then the operation ſucceeded wonderfully well; and the ſevered head will often keep the horns nearly as much extended as before decollation, only they ſink down and appear feeble; the head itſelf contracts and concentrates very much. It dilates, if ſoaked ſeveral hours in water, and becomes ſoft; in which ſtate it can eaſily be diſſected.

'I began the diſſection cloſe to the cut; and, after removing the integuments, I could diſtinctly ſee the diviſion or diſtribution of the nerves proceeding to the eyes, and the other parts of the head. The ſituation of the œſophagus, and the muſcles ſerving for the different motions of the head, were as diſtinctly obſerved. Sometimes I examined the whole or part of the ſeparated brain, inſtead of the nerves.

'By continuing the diſſection, and extending it further, I could eaſily trace the different parts, even to the places of inſertion in the head. Without the leaſt difficulty, I found the throat of the animal, its tongue, lips, mouth, teeth, the four horns, with their nerves, muſcles, and other appendages. The horns might be retracted into the head: I had only to pull the end of the divided muſcles.

'Theſe, my dear correſpondent, are the moſt remarkable parts that preſented themſelves to

'view

'view in the divided head. I say, *the most remarkable;* for I discovered many others less 'important, of which I shall treat in my large 'work.

'Let me now ask you, if this assemblage of 'parts composing the severed head, if this assemblage which I have seen, and an hundred 'times seen again, is simply the integument of 'the head, or a portion of this integument, as is 'imagined by the French observers, whose experiments you have communicated to me. Is it 'not the most satisfactory evidence, that I have 'been under no illusion, and that this severed 'head fully contained all the organs of which it 'is originally composed.

'I practised nearly the same method with regard to the head reproduced. It would be 'superfluous to enumerate the organs composing it; for I could only repeat what has 'just been said of those we discover in the original head cut off. This decollation, indeed, frequently gives place to various monstrosities of 'the parts reproduced; but the essence of my 'discovery is not affected by them.

'I have taken care to measure the severed 'head, and to compare it with that reproduced. 'Many other precautions have also been used; 'and although I omit them here, they shall be 'mentioned at large in my work.

'I flatter myself, that my treatise on the repro-'ductions of snails will be so rich in experiments, 'and that they will be so amply and accurately 'described, as to convince the most obstinate in-'fidels.'

The reader may now decide whether I had any foundation for reproaching the excess of M. Adanson's pyrrhonism concerning the discovery on snails. It is truly most singular, that he persists in his doubts, after the perusal of a letter so strictly demonstrative as that I have just transcribed. How many physical facts are admitted by philosophers, and by M. Adanson himself, which are no better established than those of which we treat. Shall I say more? M. Adanson still retained his doubts 9 October 1779, as I learned from himself during a visit he then paid me on a journey for the recovery of his health. At that time, I had no snails in full reproduction, but I had the satisfaction of convincing him, by the testimony of his own eyes, of the reality of the prodigies presented by the reproduction of the members of the water newt. I showed him newts in various stages of reproduction; I showed him arms, hands, thighs, legs, feet, perfectly well formed. He yielded to so many accumulated proofs; and was convinced that what he had erroneously supposed simple stumps were actually real members which would be completely regenerated.

I now return to my experiments on the reproduction of the head of snails. Those whose progress I mentioned in the first part of this Treatise died before finishing the reparation of the head. They became much emaciated, and assumed a transparence which is unnatural to snails. One, whose anterior part is represented, fig. 12. reproduced a large horn only, about a line in length, but much thicker than a large horn beginning to protrude, fig. 15. c. This singular horn, which seemed to be formed like a kind of spindle, had two eyes, fig. 12, *o*, *o*, very distinct, and each with its optic nerve. The whole is magnified, fig. 15: the part reproduced, which is always lighter than the original flesh, is indicated by the slight shading. On close examination of the horn, it was immediately discovered to be formed by the union of two horns, which were as if ingrafted on each other. There was no mark of a mouth, nor were the small horns perceptible in this snail: therefore, how can there be any doubt of complete decapitation (1)?

In spring 1778, I resumed these experiments on different species of snails. Their different reproductions presented varieties similar or analogous to those that I had observed in the snails decapitated the preceding year. One among them resembled that which is just mentioned. Two

 eyes

(1) There is here some inaccuracy in the original, which I am unable to correct.—T.

eyes could be very diſtinctly ſeen in the left horn of a ſnail that had begun to reproduce the mouth and the two large horns, fig. 16, *c*, *c*. The anterior part is here repreſented larger than life.

During the whole courſe of the year 1778, I continued to attend my ſnails. The progreſs of reproduction was very unequal as uſual: and none completely repaired the head.

I decapitated twenty-four ſnails of the ſame ſpecies, 26 May 1780, and confined them in veſſels after the operation. Several attained the ſummit of the veſſels, and attached themſelves to the ſides, or to the paper covering over the mouth. The greater part ſhut up the ſhell with a very thin operculum at different degrees of depth within.

Several of the decapitated ſnails being immerged in water, that I might judge of the ſtate of reproduction, 9 September 1780: two exhibited a remarkable monſtroſity. One had a ſingle large horn only, very like that of fig. 15. and was evidently formed by the union of two horns. Two ſmall ſhining black eyes were at the extremity: each provided with an optic nerve perfectly viſible through the tranſparent fleſh. This monſtrous horn, which was about a line long, appeared thicker in proportion than that of fig. 15. It greatly reſembled a ſpindle, being cut even at the extremity, and all nearly of the ſame thickneſs. But it was different from the other by a more important

portant diſtinction. A little under the eye, on the left ſide, was ſeen a very minute tubercle, which ſeemed to be a ſecond horn growing out of the large one. I ſought in vain for the parts conſtituting a mouth in this ſnail. No veſtiges of it could be diſcovered, nor was there the ſmalleſt indication of the two ſmall horns.

The ſecond ſnail exhibited another kind of monſtroſity. Only one large horn had protruded; but at the extremity I thought I could diſcern three black eyes, which were ſo cloſe, that they ſeemed confounded together, fig. 17, 18: the figures are magnified. On the upper part of the horn were very diſtinctly obſerved three parallel optic nerves, only one of which proceeded to the three minute eyes. Under this horn, and at a little diſtance from the origin, a very ſmall one was diſcovered, apparently firſt beginning to expand. Here, as before, there was no viſible indication of the parts forming the mouth.

I examined the two ſnails again on the 25 of October, both with the naked eye and a magnifier. Reproduction had made very ſenſible progreſs. Only eight of the whole ſnails were now alive, and all of that ſpecies with a yellow or yellowiſh ſhell. The other ſixteen had periſhed, ſome ſooner, ſome later.

The ſix ſnails, whoſe reproduction I had not yet examined, were immerſed in water. Moſt of

 them

them had made but little progrefs, and fhewed only the origin of a fingle horn: Two eye-horns had protruded in one alone, at leaft a line and a third in length, and the optic nerves fo large, or vifible, that they feemed to darken the greateft part of the horns; but the fmall ones, the lips, and the other parts of the mouth, did not yet appear. This fnail obftinately remained in the fhell, though immerfed in water more than two hours. Sufpecting its death, I had taken it out; and, not before an interval of feveral hours, was I agreeably furprifed, by feeing it proceed from its fhell of its own accord, and difplay its new productions before me.

In another Memoir, I fhall give the hiftory of thefe eight fnails.

After experiments on fnails of the fmalleft fpecies, it was proper to make them on the largeft. This I began to execute, 24 May 1780, on twelve of the largeft that are found with us. We may judge of their fize, compared with the fnails which have hitherto been the fubject of this and the preceding memoir, when the diameter of the mouth of the fhell of the latter was at leaft nineteen lines, and that of others, only four or five.

In a few weeks, half my fnails died, and exhaled an exceffively fœtid odour. On 13 Auguft, I immerfed the fix furviving. They proceeded from their fhells; and I faw that the im-

menfe

mense wound was perfectly cicatrised, but no indications of reproduction were perceptible.

Towards the middle of October, other two snails died. On the 18, the four remaining were immersed in water, where they remained more than three hours without appearing. With my nail, I scratched the last volute of the spiral, but in vain. All my endeavours were fruitless, and the snails obstinately concealed the anterior part; they were then taken out, and confined in their vessels. I will ingenuously acknowledge, that I had little expectation that the snails had made any reproductions. What, then, was my surprise, when they proceeded from their shells next morning, of their own accord, and exhibited unequivocal evidence of reproduction, and even of reproduction considerably advanced. One, in beginning to repair the head, had two large horns about a line in length, fig. 19. The left, *c*, which was thicker than the other, had two distinct black eyes at the extremity. The right horn, on the contrary, seemed to have none. No regeneration of the small horns, or mouth, could yet be discovered.

Another snail had likewise begun to reproduce two large horns, nearly of the same size as the preceding, but the right horn was monstrous; it apparently terminated by three small soft points, fig. 20.

 The

The reproduction of a third head was announced by four or five short protuberances, where the large left horn, bearing three black points, or very minute eyes, was alone visible. The whole is magnified, fig. 21; the three eyes, *a*, *b*, *c*. Two, instead of being at the extremity of the horn, were on the side; one, *a*, more evident than the other, *b*, and the third was a little below the other two. It was impossible to discern the optic nerves through the flesh, for the reproduction was yet in an early stage. Under the horn were two protuberances, *p*, the nature of which could not be known; but I shall continue my observations on these large snails, and give the sequel in another Memoir.

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MEMOIRS

ON THE

REPRODUCTION OF THE MEMBERS

OF THE

WATER NEWT,

BY

CHARLES BONNET.

MEMOIR I.

WHEN the prodigies of the celebrated polypus aſtoniſhed the world, M. de Reaumur ventured to predict to the Academy of Sciences, that it would not be very long before many other animals were diſcovered, which ſhould preſent the

ſame,

ſame, or analogous wonders (1). I was the firſt who had the good fortune to verify this prediction, and it was ſoon confirmed by the moſt celebrated obſervers (2). Various ſpecies of freſh-water worms, earth-worms, ſea-nettles, and ſea-ſtars, reproduced like the polypus, from ſections (3). Theſe diſcoveries were an immenſe addition to the riches of organic philoſophy, and an inexhauſtible ſource of meditation for the philoſopher. But we had not then attained their limits. New prodigies, perhaps more wonderful ſtill, were to be unveiled to naturaliſts; and to the illuſtrious Abbé Spallanzani was the diſcovery reſerved. It is already evident, that I particularly allude to the regeneration of the head of the ſnail, and the members of water newts. People have doubted, nay, they yet continue to doubt, theſe beautiful diſcoveries, and additional confirmations ſeem to be deſired by the impartial public. I have begun to give ſome in my Memoirs on the reproduced head of ſnails; and there, in my opinion, is the reality of that reproduction

(1) In the year 1740, *Memoires ſur les Polypes*, by M. Trembley. Leyden, 1744, 4to.

(2) In 1741, *Traite d'Inſectologie*, Part 2. Paris, 1745, 8vo.

(3) Reaumur, *Memoires ſur les Inſectes*, T. 6. Preface, page 49. Edit. 4to.

reproduction ascertained. In a second treatise, I proceed to relate recent experiments on the reproductions of water newts; and it will be seen that the renowned Reggian naturalist's discovery here, is no less certain than that of the head of the snail.

It is unnecessary, in this place, to describe the newts which were the subject of my experiments; they were exactly the same as those described in M. de Bomare's *Dictionaire d'Histoire Naturelle*, which is in the hands of all the world. A newt of full size is represented, plate 9. fig. 1. These animals are of a deep brown colour, covered with round or elliptical tints, almost black; the under part of the belly is dark yellow, and covered with black spots, and the skin of the sides shagreened with whitish or yellowish tubercles. Young newts are different, being of a yellowish green colour, with light brown specks, or minute lines. But I again repeat, that it is not meant to describe the animal, but only to give an idea of the wonderful reproductions which I have beheld.

I. *Method of preserving newts.*—The newts are kept in large glass vessels, full of fresh water, which is renewed at least twice a week; for it becomes turbid in a few days, and it seems injurious when too long of being changed. Only one is put in each vessel. From time to time,

they

they rise to the surface for respiration, and, after discharging several large air bubbles, immediately dive to the bottom. These little quadrupeds are perfectly innocent, and may be handled with safety. I have often held them without the smallest attempt to bite. I have even mutilated them in my hand with impunity. This remark is necessary, because there is a prejudice general among country people, that newts are very dangerous animals (1).

II. *Food.*—The newt is carnivorous; it seems to care for living insects only, and is induced by the motions of the prey to seize it. In this it resembles spiders and ant-lions, which refuse to touch dead insects.

These animals can support the want of food very long. Some of mine have lived two months without it. Sig. Spallanzani had remarked the same; and observed that, although long deprived of nutriment, they reproduced their members equally well as those plentifully supplied with sustenance.

In my opinion, no insect is better adapted for food than the earth worm: we may almost affirm, that nature has purposely prepared it for feeding various animals. There are few places where worms do not abound; and as they may be

(1) M. de Maupertuis first proved that newts are perfectly innocent. *Mem. de l'Academ. de Paris*, 1727.

be divided into various pieces, and ſtill move, they are admirably ſuited for the food of newts. Another advantage attends them, which is the property of remaining ſeveral days alive in water; and it is their motion that particularly excites the appetite of newts. A worm preſented on the end of a pencil, or dropped into the water, is ſeized with a ſudden motion of the animal's jaws, and ſwallowed alive, with gentle ſhocks of the whole body, and eſpecially of the anterior part. A conſiderable time is occupied in ſwallowing the worm, if it is large; and it ſometimes remains two or three minutes in the newt's mouth, folding in various ſhapes while life remains. The portion hanging out very much reſembles a long thick tongue; and its numerous contorſions exhibit a moſt ſingular ſpectacle.

My newts have always appeared to ſwallow their prey without maſtication; however, they have a number of very minute teeth, which undoubtedly are not totally uſeleſs; and they ſerve to retain the prey, if attempting to eſcape.

A large worm, ſeized by the middle, is ſeldom ſwallowed in the ſame poſition, becauſe it is too large if doubled in the mouth; and the newt gradually ſhakes it out, until it can ſeize one of the extremities; which being accompliſhed, the worm is ſoon devoured. However I have obſerved a large one ſwallow a worm taken by the middle,

middle, without ſeizing an extremity; but a quarter of an hour was occupied in the meal.

Though newts are provided with flexible, jointed fingers, it is remarkable that they make no uſe of the hand, either to ſeize their prey, to convey it to the mouth, or retain it there. Yet I have never ſeen it done; they never ſeemed to uſe their hands for this, or any thing elſe indeed, but ſwimming (1).

They firſt ſeem to fix their eyes on their prey, and immediately dart upon it open mouthed; and when once ſeized, it very ſeldom eſcapes. The ſucceſſive motions of deglutition are very ſenſible; it is performed by gentle ſhocks, as we have juſt remarked. I have not obſerved newts ſearch for prey; they take it when it occurs in their way, or is juſt beſide them. A newt having devoured a large earth worm, I ſupplied it with another above four inches long, and thick in proportion. It immediately ſwallowed the whole, except a line or two that hung out of the mouth; but the worm was ſoon thrown up, and the ſame repeated twice, yet the worm ſtill lived.

It

(1) Neither do toads, which have alſo articulated fingers, make any uſe of them. However, I have ſeen one ſmall ſpecies, which profited by their aſſiſtance, to retain the prey while devouring it.

It might be supposed that the amputation of the limbs is most painful, and that the animals would suffer long and severely from it: however, one of my observations apparently infers the reverse. I cut the left hand and the right foot off a large newt, and a stream of blood, as thick as a hog's bristle, continued spouting out nearly two minutes without intermission. Not only did the animal seem not in the least enfeebled by the loss of blood, but, in scarcely a quarter of an hour, to my great surprise, it swallowed two earth worms.

Sometimes worms are devoured entire, notwithstanding all their exertions to escape. They twine round the neck of the newt like a serpent: every moment they become shorter, and gradually disappear, according to the portion which enters the body. Thus have I seen a newt swallow a worm, more than six inches long, in less than five minutes.

III. *Spoliation of newts.*—It cannot properly be said that newts change their skin, for it is only the epidermis which is thrown off (1); at least, the spoil is so thin and transparent, that it seems to correspond with an epidermis only. It is of a whitish

(1) M. de Fay has already remarked this fact in his curious memoir on newts, to which I refer the reader. *Mem. de l'Acad. de Paris*, 1729.

whitiſh colour, and reſembles the fineſt gauze; indeed it is almoſt as fine as a ſpider's web. What poets have figured of phantoms may be applied to the ſpoil of newts. The whole body appears: we ſee the hands, fingers, feet and tail, but all in a ſhade floating in the water.

When the period of change approaches, the fine ſkin is obſerved detaching from the body. The head firſt loſes it; then the reſt of the anterior part; next the middle, and the poſterior part. Sometimes the ſpoil, caſt by the head, forms like a gauze collar or cravat around the neck; or it is adjuſted on the head, like a capuchin or head dreſs.

The commencement of ſeparation, from the back and belly, is diſcovered by viewing the newt obliquely from one ſide, in a ſtrong light. The ſkin of the belly is further detached, becauſe it falls down by its own weight.

Approaching ſpoliation is recogniſed by conſpicuous and unequivocal ſymptoms. The back, viewed obliquely, appears whitiſh, and as if covered with a ſpider's web. This is the effect of the ſpoil beginning to ſeparate. If cloſely examined with the naked eye, or a magnifier of ſmall power, it ſeems compoſed of minute ſcales covering the calloſities or tubercles, which ſhagreen the body of the newt. But, when examined with more attention, and in a favourable

light,

light, this epidermis is discovered to be a beautiful reticulation, the meshes of which are visible to the naked eye.

Many observations could be made on the texture of this delicate membrane; and these might greatly tend to elucidate the nature and origin of the epidermis, which, notwithstanding all the researches of physiologists, are so little known; and newts would afford frequent opportunities for deeply investigating the point.

From particular attention to the newts in my possession, I have observed, that there is not the smallest resemblance between this operation and what is exhibited by caterpillars, and many other insects. The skin is detached here and there, and often in different sized plates; and the change is slow, for it occupies one or two days, and I have even known it take three. During spoliation, the newt continues moving about in the water, with all the usual motions of newts that undergo none; therefore it is no disease, and it does not affect them as it does insects. While the change is going on, the animal darts on its prey, holds and devours it.

Sometimes spoliation is difficult to be accomplished; but, in these cases, the newt knows to practise certain manœuvres, to facilitate the operation, which I have often beheld with pleasure. It alternately raises and depresses the right arm

 and

and left leg, at the fame time with gentle vibrations of the whole body. It frequently darts fuddenly towards the furface of the water, and the next moment precipitates itfelf to the bottom; and thefe manœuvres I have feen continued above half an hour. But the fudden exertion, in all its motions, indicated that the newt was impatient at the tedioufnefs of the change.

When moft of the fpoil is thrown off, and the animal, to difengage itfelf from the reft, rapidly rifes to the furface, it feems carried along in a cloud; for the whitenefs, finenefs, and femi-tranfparency of the fpoil, floating around it, is no imperfect reprefentation of a cloud.

I never obferved the fingers employed in detaching the fpoil. Both young newts and thofe full grown caft feveral fucceffive fkins: fome of large fize are in my poffeffion, that have done fo before me. Reproducing limbs throw off the epidermis as well as the original.

I have feen the fkin of the head, which formed like a collar or cravat round the neck, gradually come down the belly of a large newt that had loft the arms, and faften like a tight girdle.

Nothing can accurately be faid of the number and interval of mutations. Between the 14 of July and the 7 of September, a newt has changed its fkin eleven times.

1 change,

1 change,	14 July.	6 change,	9 August.
2	17	7	—
3	20	8	19
4	24	9	24
5	30	10	26
		11	6 September.

Spoliation sometimes makes a slight change in the colour.

IV. *Reproduction of newts.*—It is long before I arrive at that part of the history which is most interesting, namely, the reproduction of the members of newts. My experiments began 5 June 1777; and, in the course of that and the following month, fifteen newts were mutilated, some very young, and the rest full grown. All the former died, and several whose increment was complete. As this treatise is limited to the successful experiments, those that were fruitless shall be suppressed.

I shall denominate the anterior extremities, *arms* and *hands*; and the posterior, *legs* and *feet*: circuitous expressions will thus be avoided.

On the hand are only four fingers, but there are five toes on the feet. It has already been remarked that they are well articulated, and nearly resemble our own. They neither have nails, nor are they connected by intermediate membranes; but I repeat, that my purpose is to avoid descriptions.

EXPERIMENT I. *The right arm and left leg amputated.*—On the ſixth of June, I cut the right arm and left leg off a large newt, very near the body. A ſtream of florid blood ſpouted a minute and a half from each wound; however, the veſſels ſoon cloſed, and the newt was apparently as well as thoſe unmutilated. But it will eaſily occur that it did not ſwim with equal facility.

When about a month had elapſed, I began to perceive a papilla, of a violet grey colour, near the edge of the trunk or ſection. This was the origin of a new arm and leg, which gradually increaſed; and, on the 14 of July, appeared ſuch as fig. 1, *c*, *c*, plate 9.

The two papillæ continued growing on the ſubſequent days, but more in length than thickneſs. They became minute ſtumps; and, on the firſt of Auguſt, were about two lines long. Fig. 2, 3, repreſents the natural ſize: the trunk *t*, *t*, which has grown none, and is of a much darker colour, is clearly diſtinguiſhed from the reproductions, *b*, *c*. A kind of cleft, *s*, hardly perceptible, announces the appearance of two toes, which nature labours to produce, or rather to expand, on the new foot. No cleft appears on the originating arm.

The two toes were eaſily recogniſed on the ſeventh. They were real miniatures, and truly moſt minute, fig. 5. The ſtump of the arm, nearly

nearly as it was on the firft of the month, is feen fig. 4. It is now fomewhat larger, but as yet there is no indication of fingers. Fig. 6. reprefents it on the ninth.

It is pleafing to obferve the little hand fully unfolding, while only three fingers of unequal length are vifible: the middle one is the longeft. The arm has made no fenfible progrefs. The new foot had four toes alfo of unequal length; the firft and fecond of which are longeft; the other two only begin to appear; the fourth is fcarcely perceptible. One can never tire contemplating thefe miniatures, and admiring the wonders of the organic kingdom.

Evolution advanced every day. On the 22 of Auguft, the regenerated members were as fig. 8, 9. They began to deepen in colour, fo that the line, difcriminating the old parts from the new, was no longer fo confpicuous; but the black fpecks on the toes of unmutilated newts were ftill imperceptible. The difference between thefe toes and thofe of fig. 1 is evident here. Four well fhaped fingers were already on the hand, fig. 8; but only four toes, equally well formed, of the five which the foot regenerates, fig. 9; and they have to acquire more fize, confiftence, and colour.

I continued my obfervations on the daily evolution of the members; and the following were their dimenſions in length, on the 20 of September.

Old Members.		*New Members.*	
Arm,	4 lines.	Arm,	$2\frac{1}{2}$
Cubit,	$3\frac{1}{2}$	Cubit,	$2\frac{1}{2}$
Thigh,	3	Thigh,	$2\frac{2}{3}$
Leg,	4	Leg,	$2\frac{1}{4}$
Longeſt finger,	$3\frac{2}{3}$	Longeſt finger,	$1\frac{1}{2}$
Longeſt toe,	$4\frac{1}{2}$	Longeſt toe,	$1\frac{1}{3}$

Even in the beginning of October, the fifth toe of the new foot was not viſible.

EXPERIMENT II. *A newt deprived of the right arm and left hand.*—On the 12 of June, I cut the left hand and right arm off a newt: my chief object in this experiment was to verify Sig. Spallanzani's aſſertion, that nature reproduces exactly the portion amputated, which was a fact of the utmoſt importance in the theory of animal reproductions, and could not be too well eſtabliſhed.

Towards one ſide of the ſection, a little conical nipple began to appear about the 7 or 8 of July, of a violet grey colour. An incipient cleft, indiſtinctly ſeen with the naked eye, was perceptible near the middle of July: the papilla ſeemed ready to divide in two; and the cleft was the origin of two fingers.

In

In two or three days, I remarked a new cleft at the upper extremity of the papilla, which was the beginning of a new finger: the third, in its turn, appeared on the nineteenth. The conical papilla had then disappeared; and in its place was seen a small open hand with four fingers, still very minute, but quite well shaped. It is drawn from nature, such as it then was, fig. 10. That papilla, from which originated a new arm, gradually extended: at first it was similar to the papilla, *c*, fig. 1. but towards the end of July, or beginning of August, it increased so much as to resemble that of fig. 2. exactly.

On the third of August, the cone began to divide, that is, two fingers became evident, fig. 11. Close examination is required, for the division of the fingers is hardly visible, 1. the trunk *t*.

On the ninth, a hand extremely minute, but the most beautiful object imaginable, was observed at the extremity of the arm. The fingers, all of unequal length, were distinguished, the smallest being just perceptible. This is correctly designed, fig. 12. The trunk *t*, or part of the original arm, is connected to the body. It may be recognised by the brown colour, and from being covered with white points. The new arm *b* is of a lighter and uniform colour. Four fingers of the hand, *m*, are viſible: the largeſt not above half a line in length.

The hand of the left arm had made conſiderable progreſs on the 21: it had expanded, and nearly acquired the figure peculiar to the newt's hand. The fingers alſo had extended, and become thicker in proportion. The whole hand began to colour, and brown ſpecks were diſtinguiſhable on different parts; they were more evident on the back of the hand than on the fingers. This will be better underſtood by fig. 13. than any deſcription can render it; and on comparing this with fig. 10, we obtain a more correct idea of the progreſs of evolution.

The reproduced arm is deſigned, fig. 14. as obſerved on the twenty-firſt. The hand has already aſſumed its natural ſhape, and the rapid progreſs of evolution is ſuſpending. Colouring of the arm begins near the trunk; but all the reſt is of a mixed grey and violet.

Though I have not hitherto expreſsly ſaid ſo, it will obviouſly be preſumed, that there is a kind of ſemi-tranſparency in the reproduced parts, which the original members have not. This continues long, and changes ſlowly as the reproductions colour. The tranſparency is evidently greater on the edges of the fingers than elſewhere, if examined with a magnifier, they ſeem incloſed in a fine diaphanous envelope: but nothing

thing of this is evident in the old fingers. Parts beginning to unfold naturally have a degree of transparence wanting in those further advanced, or fully expanded, because, with the progress of evolution, the calibre of the vessels increases, which allows admission to more gross and colouring particles. Whiteness and transparence apparently constitute the primitive state of organic bodies. It is this primitive state which we design by the word *germ*; and which we can comprehend, when the organic whole is expanded to a certain extent. But there is here a term beyond which we cannot ascend; for the organic whole either becomes so minute or so transparent, that it escapes all research and our most perfect instruments.

The dimensions of the old and new members, in length, were as follows, on the 2 of September.

Old Members.		*New Members.*	
Arm,	$3\frac{1}{4}$ lines.	Arm,	$2\frac{1}{3}$
Cubit,	$3\frac{1}{2}$	Cubit,	$2\frac{1}{4}$
Longest finger,	$1\frac{1}{2}$	Longest finger,	$1\frac{1}{2}$

EXPERIMENT III. *Two fingers and three toes amputated.*—I cut two fingers and three toes off a large newt, 15 July. A little elevation was perceptible on the section of each finger, on the

26,

26. At the end of each trunk, a new finger appeared, 20 August. The fingers, *n*, *n*, are designed, fig. 15. The whole magnified is more distinct, fig. 16. The fingers have yet grown very little.

The five toes are magnified, fig. 17. Considerable part of three that had lost about one half, is already reproduced, and the new production is at the extremity of the old portion, *a*, *a*. An irregular curve, separating the two parts, is very conspicuous here, and evinces that the old portion has grown none.

Experiment IV. *The right hand of a newt longitudinally divided, and two fingers amputated. The other hand cut transversely.*—On the third of August, I made a longitudinal incision through the right hand of a large newt, in such a manner as to separate two of the fingers. At the same time, I amputated the other hand in the middle, by a transverse section.

The left stump exhibited the usual conical papilla on the 22. It began to divide on the 30; and three originating fingers became distinctly visible.

On the same day, a new finger appeared on the side of the right hand. Two of the fingers had been left entire, *a*, *a*, fig. 18. A sensible eminence, *e*, on the section of the hand, preceded the

the origin of the new fingers, which is very evident, *r*. On the first of September, a second finger began to grow beside the other, and on the fourth, this hand, which had been cut longitudinally, was as it is represented fig. 19. The two fingers which replace those amputated, *r*, *r*. On the same day, the new hand growing from the left arm, was nearly as fig. 10.

This experiment is attended by a most remarkable consequence; that a hand, longitudinally divided, should reproduce exactly the amputated part, and reproduce it both in the manner and place most consonant to its natural figure and functions.

The hand was designed anew, 9 September, that one may judge better of the position of the regenerated fingers and the progress of their evolution, fig. 20.

Experiment V. *An oblique section of the hand, so that only one finger remained.*—It would be too precipitate to conclude, from the preceding experiment, that nature never reproduces more than the portion amputated, and she herself would disclaim it. Nevertheless, decisive experiments prove the fact. On the 21 of August, I cut the right hand of a newt, in such a manner as to leave only the first finger. Four very minute papillæ appeared on the edge of the section,

tion, 13 September. I could eafily recognife them for the rudiments of four new fingers. Yet, from the danger of deception, I fufpended my opinion; however, as evolution fenfibly advanced, four fmall well fhaped fingers arofe, which it was impoffible to miftake, fig. 21. Their arrangement is irregular, for the firft and fecond feem united, and the diftance between them is a little lefs than between the two laft, *c*, *d*.

Behold a hand with five fingers, though nature has given the animal only four. However, it is not uncommon to meet with fuch anomalies in thefe reproductions. Sig. Spallanzani has obferved many, which he will defcribe with his ufual acutenefs and perfpicuity. It is evident, that the place where the cut is made, the mode of making it, the ftate of the part itfelf, and of parts connected, may give birth to infinite varieties and fingularities; and fome will be real monftrofities, either by defect, excefs, or tranfpofition.

It muft be obferved, that in this cafe, reproduction was not announced by one papilla only, as in the firft two experiments, but by four very minute, and quite diftinct, ranged in the fame line, and in the direction in which the inftrument had performed the operation.

EXPERIMENT

EXPERIMENT VI. *The tail of a newt amputated transversely.*—Something important would have been wanting, had I neglected amputation of the tail, which is a very intricate great organic substance. It is formed of a series of minute vertebræ, with arteries, veins, and nerves, and it is covered with muscles and flesh.

The tail of a large newt is more than two inches long, and about half an inch thick, formed like an oar, and terminated by a soft point. Much might be said of the figure, proportions, and position of this organ, and with respect to the functions it has to exercise; but these would be details foreign to my purpose; I only mean to confirm what Sig. Spallanzani has advanced concerning the admirable reproduction of the members.

When the tails of large newts were amputated near the origin, I never succeeded in obtaining reproduction; the whole died in a certain time; and, for several weeks preceding death, a kind of whitish cotton mould grew on the wounds, the filaments of which were several lines in length. Nevertheless, I cannot think that this affected the animal's life; for I had seen similar mould, or cottony filaments, on wounds occasioned by amputating the arms and legs. These filaments gradually disappeared, and unequivocal signs of reproduction

reproduction soon became visible. Thus a good observation was never obtained, unless the tail was divided about the middle, and by a section perpendicular to the axis. A stream of blood, as thick as a hog's bristle, always spouted from the wound. The large vessel, from which it flows, is situated near the vertebræ, and its orifice is visible by the naked eye; it immediately closes; and the orifice is distinguished by a reddish or brownish point.

The tail of newts is very sensible, which is particularly evident in the slenderest part. A portion cut off will retain life, and move whole hours; and when life seems entirely extinct, we have only to prick the pointed extremity, that motion may be renewed; it rises and falls alternately, and with greater force, according to the period that has elapsed since the operation. The motion of this separated part bears great resemblance to that which is peculiar to certain apodal worms; it is undulatory, and evidently depends on irritability, which is extremely active in so muscular an organ.

Immediately after the operation, the area of the cut exhibits a very long ellipse; the two extremities almost terminating in a point. The smallest diameter is about a line across, and the largest five or six. In the centre, are the verte-

brae,

bræ, or blood veſſels; the reſt of the area ſeems full of ſmall oblong clear white ſubſtances, which one would ſuppoſe pieces of fat, or glands. The ſurface ſlowly contracts; the oppoſite ſides approach; the colour of the ſubſtances becomes fainter, and in a certain time, which is according to the ſeaſon, new fleſh appears, and it daily increaſes. Then we obſerve one or two croſs brown lines, occupying the middle of the new tail, which indicate the ſite of the vertebræ and the veſſels. The firſt evolution is deſigned, fig. 22; the part reproduced, *n*, is thinner and more tranſparent than the reſt; the brown line, *e*. At the extremity, is a ſmall internal cavity, *m*, very perceptible in the figure, which I have uniformly found in reproducing tails. This tail had been divided on the 11 of July; and on the 14 of Auguſt the reproduced part was about three lines and a half long, and four and a half in diameter, at the baſe.

The new portion was ten lines in length, 20 September, and ſhaped exactly like the tail of a newt. I could obſerve no difference between the motions of this regenerated tail, and thoſe of tails unmutilated. It was again deſigned, 8 October, fig. 23; the reproduced parts, *r*, *r*. The edges of the old trunk had then grown none; they are clearly diſtinguiſhed, *t*, *t*.

Thoſe

Those of the regenerated part had a peculiar transparency, wanting in the rest of the tail (1).

General results and reflections.—Other experiments have been made on newts, which confirm the first, but I pass them in silence, to avoid repetitions and the multiplicity of details. What I have said, seems sufficient for my principal object, which is only to establish the discovery of my illustrious friend the Abbé Spallanzani. When his large work appears, naturalists will there behold with astonishment, the various prodigies for which we are indebted to the singular sagacity of this celebrated observer, and of which this little tract can give but a very imperfect idea. So much inferior to the subject do I find my own experiments, that I never should have thought of publishing them, had not new confirmation been desired, and had not the Abbé himself wished my testimony in addition to his. I next proceed to deduce some general results

(1) Had not the fear of fatiguing my eyes too much deterred me, I should have endeavoured to compare the new limbs with the old, by dissection, and the same with respect to the head of the snail. But, in my opinion, the simple narrative of facts, or inspection of the figures, are sufficient to demonstrate the reality of the reproductions, and the similarity of the new members to the old.

refults from my experiments, which fhall be limited to thofe directly flowing from facts.

The firft, which is evident, refpects the time employed by nature in preparing for the reparation of mutilated members. In the arm polypus, and frefh water worms, which may alfo be multiplied from fections, reproduction is immediate, and in one or two days, during fpring or fummer, there are fenfible indications of regeneration. It is very late in newts, on the other hand; and only, after an interval of feveral weeks, do any fymptoms of renewal appear: thus, in the newt, fig. 1. it was imperceptible till about five weeks after the operation; and a month was required for preparing the reproduction of the newt mutilated 12 June. Polypi and frefh water worms are gelatinous; they neither have bones, nor any part that can offify. But it is by no means the fame with newts; for they are actually fmall quadrupeds, and, as quadrupeds, have bones covered with mufcles and flefh. All thefe parts exift in a gelatinous ftate, previous to their appearance. This gelatine, however, unqueftionably oppofes more refiftance to the power which effects increment, than is done by that of polypi and frefh water worms, for the ftructure is more complicated. Thus we fhould not be aftonifhed at the flow progrefs of evolution in large newts; in this cafe, it is even more immediate than when the newt is old, as the Abbé Spallanzani has already obſerved, and as I myſelf have ſeen. The reaſon is evident; for the younger an animal is, the more ductile or pliant are its ſolids, becauſe their fluids are more abundant. Irritability has alſo greater energy in young animals, becauſe they are more gelatinous. We have ſeen in the Memoir on the reproduction of ſnails, that a conſiderable time is neceſſary before the new part becomes perceptible.

The ſecond fact demonſtrated by my experiments, reſpects the firſt appearance of the members. A conical papilla ariſes, which, compared to a vegetable bud, we may call an animal bud. But the compariſon muſt be extended no farther, for the vegetable bud is properly only the envelope of a plantula, whereas this animal bud is the member itſelf, infinitely contracted in miniature. The fact is evinced, by attending to the progreſs of evolution. The ſummit of the papilla divides in two, and we immediately diſcover that diviſion is produced, by the ſeparation of two fingers, formerly united or confounded together in the ſame organic maſs; the like enſues with all the fingers, which ſucceſſively appear. The papilla is therefore a real hand, or foot, already formed; but the concentration, minuteneſs, and tranſparency, prevent it from being recogniſed in its original ſtate. Yet it muſt be obſerved,

ſerved, that reproduction of the tail is not accompliſhed exactly in the ſame way as that of the other members; it is not announced by a papilla riſing in the centre, but by a thin ſemi-tranſparent plate, extending over the whole, or moſt of the ſection, and very much reſembling an edge-tool in figure.

From a third fact, it ſeems to reſult, that the members replacing thoſe mutilated, are not properly *generated*, but that they originally exiſted in miniature in the great organic whole, and are only now unfolded. This we are obliged to admit, on conſidering that the animal bud is the member itſelf, already formed, and requiring nothing but ſize, ſtrength, and colour, to bring it to perfection. Therefore, it is probable that the reproduced members pre-exiſted in germs of exceſſive minuteneſs, and that all their parts were there. The ſpecies, proportion, and poſition of theſe, which I may call *reproductive* germs, regulate the kind, the manner, and the place of reproduction. Here it is needleſs to demonſtrate how unphiloſophical it would be to reſort to formations purely *mechanical*, for an explanation of theſe admirable reproductions. My method of philoſophiſing on this important ſubject is known; and it gives me great ſatisfaction, that Sig. Spallanzani's numerous and beautiful experiments on infuſion animalcula and animal reproduction,

 confirm

confirm the principles which, above thirty years ago, I had adopted, concerning the origin and evolution of organic beings. The work, which that excellent philofopher has lately publifhed, teaches us what our real fentiments fhould be of the *vegetative powers* and *organic molecules* of celebrated modern Epigenifts. Not only has he rigorously demonftrated the falfity of their hypothefes, by infinite various and correct experiments, but he has difcovered the caufe of error, and fhewn what the authors fhould do to avoid it. I cannot too earneftly exhort naturalifts to read and reflect on this beautiful work, which I may juftly confider one of the moft perfect models of the art of obfervation (1).

In the fourth place, it is evident that nature in general reproduces exactly the portion amputated; thus, when a hand is cut off, nature renews a hand only; if an arm is amputated, fhe regenerates one with all its parts. But, as already remarked, there are various exceptions to this law, and the fifth experiment is a ftriking inftance of it. Sig. Spallanzani will defcribe many more fingular, which his long experiments have given him an opportunity to obferve. It may well be conceived, that it is not difficult to

(1) Tracts on the Natural hiftory of animals and vegetables.

to produce numerous artificial kinds of monſtroſity, and that theſe may throw much light on the theory of animal reproductions. Certainly theſe wonderful operations are regulated by laws which depend on the nature and relation of various organic wholes, and it is the inveſtigation of ſuch laws that ſhould chiefly occupy the philoſophic naturaliſt. Some will be found more or leſs general, or ſpecial; ſome ſubordinate to others, and all to a more comprehenſive law, that regulates the whole organic ſyſtem. Here nothing happens by chance; all has been weighed, calculated, and combined, with reſpect to poſſible occurrences, and there is not the ſmalleſt alimentary atom in this wonderful ſyſtem of organs, without its proportions, its motion, its place, and purpoſe. Thus, what we denominate anomalous, or monſtrous, is the neceſſary conſequence of the admirable laws which govern the organic world, and of courſe a confirmation that ſuch laws exiſt.

There is a fifth reſult to be obſerved. When only a hand is amputated, what ſucceeds it is at firſt much larger than what unfolds at the extremity of a new arm. This is evident by comparing fig. 10 and 12. In the germ prepared for reproducing an arm and all its accompaniments, the integrant parts of the hand ſhould be ſmaller than in the germ, which contains no more than the reparatory elements of a hand;

 at

at leaſt, obſervations ſeem to indicate this, for the conical papilla preceding an arm, is no larger than that preceding a hand. Probably the body of a newt includes a number of germs of different kinds, appropriated to the various reproductions that are to enſue, and each germ is placed in the ſituation and manner beſt correſponding to its evolution. But I ſhould refer to what is ſaid on the ſubject, Part 9. 10, of *La Palingeneſie*.

A ſixth reſult ariſes from the evolution of the fingers and toes. It is not effected in the ſame proportion as evolution of the arm and leg. Now, when I write this, the tenth of October, the new arm and leg of the newt, which was mutilated 6 June, have nearly attained the ſize of the original members, while the regenerated fingers and toes have not acquired half their natural ſize; yet they are perfectly well formed, and execute all the functions peculiar to theſe parts.

The ſeventh and laſt reſult is preſented by the trunk of the mutilated members. While the reproducing part unfolded, I never obſerved any elongation of the trunk. In this reſpect, there is a coincidence with the ſtate of the trunk of earth worms, and of thoſe freſh water worms which I multiplied by ſections thirty-ſix years ago; and the truth of this obſervation may be judged of, by inſpecting fig. 2. 3. 10. 12. 17. 22. 23. plate 9. The ſame enſues in regeneration of ge 390.

of the ſnail. When the fibres of an organiſed body are indurated to a certain degree, they are no longer ſuſceptible of extenſion; there is a term beyond which the elements of the ſolids cannot glide along each other. This reſult affords an opportunity of obſerving, that it evidently concurs in proving the part to be actually a new organic whole, which expands on the old one, and is in a manner ingrafted on it. In another treatiſe, I have inſiſted much on the point.

Perhaps this Memoir may be followed by another, containing the ſequel of my experiments. I intend to diverſify them greatly, and thus give birth to new facts by new combinations. But it muſt be remembered, that all which it is in my power to do, will even be infinitely inferior to what the public may expect from the learning, ability, and induſtry of the celebrated Reggian philoſopher. No naturaliſt has equally enriched the hiſtory, ſo new and intereſting, of *Infuſion Animalcula* and *Animal Reproductions*; and I may predict, that his account of the generation of animals and vegetables, will not be leſs valuable. How impatiently would the admirers of natural hiſtory expect theſe new fruits of the indefatigable obſerver's labour, did they know, as I do, the important and unexpected truths which they contain.

MEMOIR II.

My observations were continued on the newts, whose history I have given in the preceding memoir. That which was mutilated on the fifth of June 1777, and the subject of my first experiment, died on the tenth of December; but what occasioned its death, I am ignorant. The limbs increased and coloured: the fifth toe was wanting; indeed I did not expect it after the beginning of October.

The newt, mutilated 3 August, which is mentioned in the fourth experiment, passed the winter safely in my apartment, and lived until 8 April 1778: it then died, probably from neglect to renew the water. The two new fingers, longitudinally divided, had not acquired above half their natural length.

The newt of the sixth experiment, whose tail was mutilated 11 July, shared the same fate as the preceding. It died towards the middle of November. The tail had extended very much; and the new part could scarcely be distinguished from the old, except by the lighter colour.

I had

I had more reaſon to regret loſing the ſubject of my fifth experiment, that is, the newt which had loſt three fingers and reproduced four. The monſtroſity rendered it precious. It had been carefully preſerved during winter, and was in perfect health in the following ſpring; therefore I cannot tell from what cauſe it died. Sig. Spallanzani has proved, that worms are found in theſe little quadrupeds; perhaps their numbers might occaſion death. Whatever it was, the four new fingers had grown conſiderably, and become dark coloured; but their arrangement was equally irregular as at firſt.

Here I may remark, that it is unneceſſary to change the water as often in winter as during ſummer. That in a veſſel, containing one large newt, has remained eight days pure in winter; while it has become turbid in two or three, and ſometimes ſooner, during ſummer. Newts then perſpire, and evacuate more copiouſly, and are alſo more voracious. The ejecta are grey filaments, or flocks, floating in the water (1).

It is incredible how long the new fingers require to attain the ſize of the old. I have had newts, whoſe fingers, in thirteen months and more,

(1) Newts frequently void ſmall round or oval ſubſtances, of a brown colour, but of very little conſiſtence, Some are at leaſt as large as a pea.

more, were not as large as thofe of unmutilated members.

In general I have fuppofed it was otherwife with the arm and thigh reproduced, and with the cubit and leg; for the former, in both cafes, fooner acquire their proper fize, as is already remarked in the fixth refult of the preceding memoir. The like fucceeds with the tail. But all thefe reproductions are very flow, when compared with thofe of polypi and frefh water worms; reafons for which are affigned in the firft memoir.

I fhall now proceed to my new experiments on quadrupeds, fo worthy of the attention of naturalifts.

Experiment I. *Whether reproduced members poffefs the fame fources of reproduction as thofe amputated.*—I cut the left arm and right thigh off a large newt, 2 June 1778. In the beginning of July, a new arm and thigh began to reproduce. They were ftill in miniature, but the fingers and toes fufficiently formed, and very diftinct. They were nearly as thofe of fig. 6, 7, plate 9.

On the eleventh of July, I made an experiment, which was moft important in the theory of animal reproductions. The object was to difcover whether the members now reproducing, which in reality were miniatures, contained the fame fources of reparation as the original; that is, is, whether there were, in new limbs, germs containing members in miniature, ſimilar to thoſe amputated. With this view, I cut off the regenerated hand and foot.

At the extremity of the reproduced leg, on the 21, appeared two new toes extremely minute, but eaſily recogniſable by the naked eye: and, on the 24, an originating hand, with three well ſhaped fingers, appeared at the extremity of the new arm.

The foot, now reproduced, exhibited four very diſtinct toes. Both theſe and the fingers were yet only one fourth, or one third, of a line long.

Therefore it is proved, by this firſt experiment, that the reproduced limbs of a newt can make new productions, in the ſame manner as the old ones can, and give birth to members which, in their eſſential parts, reſemble thoſe amputated, and are different only in ſize, conſiſtence, and colour: for, as was remarked in my former memoir, the new members are of more delicate texture, and of a much lighter colour than the old.

It was undoubtedly moſt intereſting to aſcertain how far the reſources of nature extended; and whether, after ſeveral ſucceſſive mutilations of the reproduced member, a new one would ſtill regenerate.

On

On the 31 of July, for the ſecond time, I cut off the reproduced hand and foot of my newt; the fingers and toes being then about a line long.

Two new fingers and toes appeared at the extremity of the limbs, 13 Auguſt; therefore a hand and foot had begun to regenerate. On the 15, there were three fingers and toes already well formed, though very ſmall.

Both the hand and foot ſeemed quite repaired on the 24, though ſtill of extreme minuteneſs. All the fingers had grown, but only four toes. And it may now be obſerved, that the appearance of the fifth toe is conſtantly later; often it does not unfold.

On the ſame day, 24 Auguſt, I amputated the reproduced hand and arm the third time; and, on the 13 of October, performed the fourth operation: the limbs being then in the ſame ſtate as thoſe mutilated by the third amputation.

Thus it is fully eſtabliſhed, that every member, ſucceſſively reproduced, contains new ſources of reparation; and that they are actually exiſting, though the member is extremely minute.

From theſe ſucceſſive mutilations of reproduced members, I have thought the extremity of the leg and arm became a little thicker than uſual, as if from a reflux of the nutritive fluids into the extremity, by ſuch repeated amputations.

This

This ſeaſon was particularly favourable to my experiments, being always dry and warm. A mercurial thermometer, in the ſhade, completely iſolated, on a large terrace, ſtood at 90° and 93°, on the 14 and 15 of Auguſt. Moſt of ſummer it ſtood between 79° and 81°; and the temperature of the apartment, where the newt was kept, differed very little from that of the open air.

Experiment II. When a large newt was treated as has juſt been related, I made another experiment on one of ſimilar ſize, to obtain comparative reſults.

The left arm and thigh were ſevered 2 June 1778. Reproduction of new members commenced in the beginning of July: two well ſhaped toes were then on the foot. On the 11, new limbs had replaced the old; they ſeemed completely repaired: ſtill they were only miniatures of moſt delicate texture. This day I amputated the reproduced hand and foot.

A new foot, with two diſtinct toes, was perceptible on the 22; and three were viſible on the 24. But the new hand had not appeared; at leaſt there was no evidence of originating fingers. The thermometer now ſtood about 84°. However, a new hand, with three perfect fingers, was ſeen on the 29.

The

The reproduced hand and foot being a full line long on the 31, I then cut them off. Both appeared again 15 Auguſt, with three well ſhaped fingers and toes. On the 24, the hand had acquired its four fingers, and the foot five toes, all viſible, though exceſſively ſmall.

I then cut off the hand and foot for the third time. The fingers and toes were a full line long 13 October; four of each appeared, but the fifth toe was yet imperceptible.

Next I performed a fourth amputation: it alſo was followed by reproductions. Various occupations having interrupted me, a fifth amputation was not made before 26 Auguſt 1779.

The longeſt finger was then about one line and a third, the longeſt toe one and a half in length, deep coloured and very ſlender. The hand had four fingers; the firſt and fourth imperfect. The foot had only three toes, more diſtant from each other than uſual. Both the fingers were as imperfect 30 October 1780: the fourth ſcarcely viſible, and conſiſting only of a ſhort point; and no more than three toes on the foot. The newt had then diminiſhed greatly in ſize, and was very brown. It ate little, and ſeldom: it remained long at the ſurface, unable to get to the bottom of the water; and its belly was almoſt always very much inflated.

These

Theſe are two experiments, therefore, which concur in eſtabliſhing the ſame fact, namely, that the reproduced members of a newt, though ſtill in miniature, are equally provided with reparatory germs as the old limbs; and that they begin to unfold after the new members are cut off.

EXPERIMENT III. *A foot cut obliquely, and a hand longitudinally.*—Experiments ſuch as I relate cannot be ſufficiently diverſified. The place and mode of ſection muſt have a certain influence on the place and mode of reproduction. Undoubtedly the germs, deſtined for theſe prodigies, cannot be fortuitouſly diſſeminated in the members. It is much more philoſophical to believe, that they are arranged in an order which we ſhould admire, if our moſt powerful magnifiers could bring them into view. But we are ſtill incapable of penetrating the ſecret organization of an animal: all that is permitted us to do, is reduced to a few experiments on what the mind can comprehend. Chiefly with this view, have I amputated the limbs of newts in different ways; that is, ſometimes tranſverſely, ſometimes more or leſs obliquely, and ſometimes longitudinally. And the detail of an experiment is now to be given, executed in the ſecond and third manner, as enough has been ſaid of the firſt.

On

On the 29 of January 1778, I cut the left foot of a large newt obliquely, leaving only the firſt toe. The four amputated toes were renewed 5 June; but not exactly in the ſame poſition as when the ſection was perpendicular to the length. The extremity of the foot was a little ſwelled, fig. 1, plate 10: the toe preſerved, 1; thoſe reproduced, 2, 3, 4, 5; the ſwelling of the foot, *r*. To judge of the difference ariſing from the two modes of operation, this figure muſt be compared with fig. 9, plate 9.

On the ſame day, I endeavoured to divide the left hand of the animal longitudinally through the middle, extending the ſection as far as the arm. Mould appeared on the wound in a few days, and made rapid progreſs. Part of the member at laſt mortified, and three fingers diſappeared.

The arm and hand were, as repreſented fig. 2, on the 5 of June: the finger preſerved, 1; thoſe reproduced, 2, 3, 4, which are not quite in the ſame poſition as obſerved in a tranſverſe amputation; the extremity, *r*, is ſenſibly ſwelled.

It ſhould be obſerved, that in this experiment, as well as in almoſt the whole related, nature reproduced an equal number of parts as thoſe amputated; which is a fact well deſerving conſideration.

EXPERIMENT

EXPERIMENT IV. *The limbs of a newt cut longitudinally through the middle.*—I divided the right hand and foot of a newt longitudinally, 16 June 1778. In a few days, the wounds were covered with whitiſh mould, which grew rapidly. It gradually became thicker and longer, and, as I foreſaw, announced the loſs of the limbs. I removed it ſeveral times with a pencil, but it conſtantly returned. This ſingular production requires more profound inveſtigation than I was able to beſtow on it. It evidently conſiſted of extremely delicate filaments, ſimilar to what characteriſe the mould originating on humid animal and vegetable ſubſtances. Sometimes the filaments grew half an inch long, and even more. Something ſimilar occurred in the freſh water worms, multiplied by ſections, in 1741, 1742; and I then conſidered the mould as a precurſor of gangrene. While obſerving it on the newts, the ſame idea recurred; and I thought of anointing the wounds with a pencil dipped in a ſolution of bark, but I cannot affirm that this was more efficacious than the former plan reſorted to. Whatever was the caſe, notwithſtanding all my care, the third toe and ſecond finger fell off about the end of June. Longitudinal diviſions certainly occaſion the greateſt diſorder in the parts, and particularly in the veſſels;

 at

at leaſt, the wound is very large, and a great internal ſurface laid open.

The parts of each member, ſeparated by the operation, were fully united, and the wounds cicatriſed 26 July. The cicatrices had even entirely diſappeared.

I now obſerved a ſmall ſemi-tranſparent grey button in the middle of the foot, which announced the finger deſtined to replace that which had fallen off. Fig. 3, *n*, the button or papilla. The foot is a little larger than when in the natural ſtate; and the new toe originates exactly in the proper poſition for replacing that which was deſtroyed by gangrene.

The divided hand is repreſented as it was 2 July, fig. 4. There is no indication of a cicatrice, all is ſo perfectly healed. The new production to unfold is ſtill imperceptible.

Againſt Auguſt 11, the new toe had made great progreſs; and the papilla had aſſumed a figure which rendered it impoſſible to be miſtaken for a real toe, fig. 5, *n*.

The hand is as it was the ſame day, fig. 6: the origin of the finger to replace the loſt one, *n*. But the newt died on the 14, without my being able to diſcover the cauſe of its death.

Experiment V.—On the 27 of July, I repeated the preceding experiment on the left hand and

and foot of a large newt. Mould did not fail to vegetate on the wounds, and, in three days, was so abundant that the members seemed endangered. However, I accomplished the removal of great part with a pencil: but, alarmed lest I should be unable to preserve either half of the members, I resolved to amputate the moulding part by a transverse section.

In several weeks, two papillæ, announcing the reproduction of two fingers, arose on the transverse section; next arose another, on a similar section of the foot, which indicated the origin of a new toe. The enormous wounds cicatrised so well, that no vestige of them remained.

The papillæ continued growing during the subsequent weeks; and, on the 15 of September, the hand had two fingers about a line and a half long. Their position was such as to give the hand its natural shape, but they were of a lighter colour than the old fingers.

The new toe was a little longer than the largest finger, being about two lines in length. The fifth did not appear, nor was it visible on the 26 of October.

EXPERIMENT VI. *A newt deprived of the third and fourth finger of each hand, and the five toes of the left foot.*—Among the beautiful phenomena which the reproductions of newts exhibit,

 one

one of the most striking is the constancy with which nature reproduces the same number of parts as those taken away. This is particularly true with regard to the hands. If three fingers are amputated, other three will be reproduced: if two are severed, only two will originate. I have already observed, that the consequence is less certain in mutilations of the foot; and it frequently occurs, that four toes are regenerated when five are amputated. But I can form no conjecture on the cause of this irregularity.

I cut the third and fourth fingers off each hand of a large newt, and the five toes off the left foot, 31 July 1778. All the fingers and toes were reproduced, and grew as usual from minute papillæ: they gradually extended, and assumed the figure and proportions peculiar to fingers and toes.

On the 27 of October, the left hand only was designed, but not the right, because it was quite similar, fig. 7, *n*, *n*, the new fingers; although these were well proportioned and regular, they had not attained half the natural size.

It was different with the toes. The three last, indeed, *c*, *d*, *e*, fig. 8, grew in their natural place; but the other two, *a*, *b*, came above the rest, so that the second was above the third instead of being by its side. The figure of the foot was very much changed; it was larger or swelled. I mention

mention these monstrosities, because they are not indifferent in the history of reproductions.

EXPERIMENT VII. *The reproductions of newts are retarded by cold.*—It is well known how much the evolution of all organized bodies is promoted by heat. Animals vegetate like plants, and heat powerfully advances their vegetation.

Thirty-seven years ago, experiments on polypi, and different species of fresh water worms, demonstrated that the reproduction of these singular animals was greatly retarded by cold. If the mutilation was towards the end of autumn, and particularly if in winter, reproduction required several weeks; and, on the other hand, it would be complete in a few days, in summer. My experiments on newts confirmed the fact in a most conspicuous manner, as will immediately appear.

On the 6 of September 1777, I cut the arm and thigh off a large newt, very near the body, and kept it in an apartment without fire until the month of January. Being then afraid of injury from the cold, I carried it to my own chamber, along with other newts, which were the subject of various experiments. All passed the winter successfully. The temperature of the chamber was commonly about 43° or 45°; but it frequently diminished 5° or 6° during night.

 The

The new members were no longer on the 6 of March than they would have been in six weeks, or two months, in summer, fig. 9, 10, the originating arm *b*, the thigh *c*. Thus they were only two stumps, after an interval of six months.

Two fingers were growing at the extremity of the arm, 9 April, and three well shaped toes perceptible at the extremity of the leg, *b*, *c*, fig. 11, 12. The fourth toe, just begun to appear, fig. 12, corresponds with fig. 7 of the first memoir.

In the following month the newt died, which interrupted my observations on the course of reproduction.

EXPERIMENT VIII. *The tail of a newt divided longitudinally.*—We have seen the effect of a transverse section in the sixth experiment of the preceding Memoir. It has already been remarked, that the tail is formed of a series of small vertebræ or ossicles, and has blood vessels, nerves, flesh and muscles. Therefore this is a very complicated member; and we immediately comprehend how admirable its reproduction from a transverse section must be: for not only are the vessels, nerves, and muscles then reproduced, but likewise osseous parts of intricate structure articulated together, and playing on each other. Yet how many parts, both soft and hard, of much

more

more complex ſtructure, are in the ſame manner reproduced in the reſt of the limbs with equal facility and regularity. Thus it may eaſily be conceived, if the ſkin of the tail is ſlaſhed, if various deep inciſions are made into it, and ſlices of different ſize cut out along the vertebræ, that nature has only to heal the wounds, and repair the loſs of the pieces. This I have myſelf beheld; and ſhall relate but one inſtance.

A ſlice about an inch long, and two lines broad, was cut from the tail of a large newt, 7 Auguſt 1778. The wound immediately healed; and from the 15, I ſaw a thin tranſparent ſlice all along the length, appropriated to replace the part taken away. In a few weeks, reproduction was complete, and the regenerated portion could not be diſtinguiſhed from the reſt of the tail.

REFLECTIONS.—If theſe experiments are compared with thoſe of the preceding Memoir, it is evident that they all coincide in confirming the reſults and concluſions relative to many phyſiological facts found in nature. Among theſe is the pre-exiſtence of germs deſtined to repair loſt members.

When, with the view of explaining this regeneration, we recur to the *powers of relation*, an *expanſive* or *vegetative power*, an *eſſential force*, *internal moulds*, and *organic molecules*, we certain-

 ly

ly use very scientific expressions, but to which no distinct idea can be attached (1). It is not enough to say, an effect is produced by a certain power: it must be demonstrated how we conceive the effect depends on the power: and, by admitting the existence of the cause, to give a satisfactory explanation of the principal peculiarities which the effect presents. Now, when we say a leg or an arm is produced by the *vegetative*, or any other power, who can be sensible how the existence of the limb naturally arises from supposing the power? Physical powers do not present their own limits: for any power is in itself indeterminate. How could the imaginary vegetative power determine the production of a leg rather than an arm, which it might just as readily

(1) The *powers of relation* were conceived by M. de Maupertuis. His ingenious work, *La Venus Physique*, must be consulted for understanding how he uses them in explaining the mysteries of generation.

The *vegetative* or *expansive power* was created by Mr Needham.

M. Wolf, Professor of Anatomy in Petersburgh, endeavoured to introduce the *essential force* into organic nature. An account of his hypothesis may be seen, Art. 334, *Corps Organisés*, Note.

Organic Molecules are M. de Buffon's favourite hypothesis.

readily produce? How could it give each part of the leg that figure, proportion, and ſtructure, which conſtitute the member? How could it arrange all the parts in that poſition and relation, in virtue of which all conſpire to the ſame end?

To affirm that a certain expanſive force, reſiding in the trunk of the old member, extends the veſſels, nerves, muſcles, and bones, is ſaying nothing at all: for it is evident, that only a ſimple ſtump or fleſhy cone would reſult from ſuch elongation. But could the cone have a hand or a foot at the extremity provided with fingers and toes? Could all their articulations be preſent; and could the ſame order and proportions prevail? Let imagination extend all the fibres of a bony ſubſtance, nothing will ever enſue but a ſimple oſſeous cone, ſo much the larger as imagination has rendered the force more powerful, and the expanding fibres more ductile. Could the extremity be faſhioned into a certain articulation? or how could an articulation, which infers certain figures, and theſe frequently very intricate, ariſe from the ſimple elongation of ſtraight or nearly parallel fibres? How could an expanſive power change their direction, and diſpoſe it ſo as to produce a joint, and alſo produce thoſe glands in the joint which are to lubricate its parts?

What

What I now observe of the osseous parts obviously applies to those that are soft. How, for example, could extension of the fleshy fibres of a newt's tail produce that infinity of glands that cover it; and secrete the viscous fluid by which it is lubricated? How could extension of the fleshy fibres, at the root of a snail's horn, produce a new horn, and place an eye at its extremity, provided with an uvea and three humours (1)?

I shall omit the *essential force*, which a very modern Epigenesist has gratuitously ascribed to matter. It would be equivalent to admitting the *plastic nature* and *creative mind* of Redi and Hartsoecker. Besides, the same objections may be made to it as to the *expansive power*. These are expressions which enrich dictionaries, but add nothing to physiology. Here I must refer to Part 11 of *la Palingenesie*.

Nothing need be said on *organic molecules*, as an excellent observer has rigorously demonstrated their non-existence, by a beautiful series of experiments

(1) It is to Swammerdam's amazing dexterity in dissection that we owe our knowledge of the structure of the eye of the snail. *Vide* his excellent history of the animal in his Biblia Naturæ, of which there is a French translation. *Collection Academique*, Tom. 5.

periments and observations very much diversified.

With regard to *internal moulds*, I think as unfavourably of them as of *plastic forces*. It must be remarked, that what are called moulds, are not simple cylinders or solid cones, but hollow tubes, nice textures, and the like. But, independent of this difficulty, where could be the internal mould of a hand, a foot, an eye, or brain, which no longer exist in the animal, but will nevertheless be completely reproduced? Do not let us confound the secretory organs with moulds; to speak correctly, they form nothing; they only separate certain molecules for certain ends.

Thus, because I am unacquainted with any explanation merely mechanical, that will account for the leading phenomena, I admit, that in the interior of the members of the newt, there are germs destined to repair the lost parts.

My experiments seem to prove, that there are different classes of these germs, and that they are not fortuitously dispersed in the interior of the members, but distributed in regular order, relative to the different possible privations.

That there are various species of germs, seems to be ascertained, from attending to what ensues in the reproduction of the hand and foot, compared with what succeeds in reproductions of the fingers

fingers and toes. A conical papilla becomes perceptible after amputation of the hand, which continues to encrease. Two fingers appear at the summit, then three, and at last four. The papilla, in its origin, was the germ; therefore, the germ contained the four fingers and all their concomitants. But should one, two, or three fingers be amputated, one, two, or three conical papilla, much smaller, will arise, and gradually assume the figure peculiar to fingers. Therefore, these smaller papillæ were originally smaller germs than those containing the whole member, and each contains only a single finger. But this is speaking incorrectly; they do not properly contain a finger; they are the finger itself, contracted and concentrated into the smallest possible space. It is the same with the reparatory germs of the hands and feet; they are themselves hands and feet in extreme concentration. But the arms, thighs, and legs, are equally reproduced; therefore, there are germs which do not include the hands and feet alone, but also an arm or a leg.

In the same way, do I admit reparatory germs for replacing joints; for I cannot conceive the simple mechanical formation of a joint, more than of a whole finger or hand.

What I observe with respect to the reproductions of newts, should apply to those of snails.

We

We have only to reflect on all that is ſaid of the regenerated horns and eyes, and attend to their admirable organization.

It is evident, that we need not recur to germs, properly ſo called, for explaining the reproduction of a cutaneous or muſcular fragment. In the ſkin and muſcles, are numerous fibrilli, appropriated to repair theſe parts, and they unfold only when certain accidents direct the nutritive juices towards the reparatory fibrilli around the edge of the wound. Large animals, and even mankind, preſent many remarkable inſtances of ſimilar reparations that ſucceed in the oſſeous parts.

In another place, I have explained myſelf at length on the various kinds of animal reproduction. There I have aſcertained the different meaning that may be applied to *germs*, and explained the principles which ſeem moſt applicable to each ſpecies of reproduction (1). The enlightened and philoſophic reader will decide between theſe principles and thoſe which are oppoſed by our modern Epigeneſiſts. Undoubtedly, he will not object the numerous germs that never unfold in newts, and other animals which repair their loſt members, for he is not ignorant that the SUPREME WISDOM has proceed-

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(1) *Palingeneſie*, Part 10.

ed on a general plan in calling the different families of organized beings into exiſtence, or on plans that may comprehend all the individuals of each family. How many millions of ſeeds, how many millions of eggs produce nothing, yet every egg and every ſeed includes a minute organic whole, which never unfolds, though deſtined for evolution. The philoſopher will not haſtily conclude, that the exiſtence of theſe organic wholes is uſeleſs, becauſe it muſt inſtantly occur to him, that his knowledge is not ſo great as to diſcover all the uſe of beings, and becauſe he will eaſily conceive, that what is not appropriated to its particular uſe in this world, may be ſo in the next (1).

Experiments on newts, ſnails, earth worms, and the like, ſeem to indicate, that the original and primitive figure of germs is ſpherical or elliptical; at leaſt, this appears to reſult from the figure in which the members are firſt ſeen. In the beginning, they are very minute roundiſh buds, which gradually aſſume another ſhape that removes them farther and farther from their original one. The wonderful metamorphoſis of the chicken, before it attains its perfect ſtate, may enable us to judge of thoſe which the limbs of newts and other regenerating animals undergo,

(1) Conſult, Part 1, 2, 3, 4, 5, of the *Palingeneſie.*

go, before they appear fully evolved (1). But we have no means of discovering organic wholes so minute, and of contemplating their successive evolutions.

Finally, the experiments on newts demonstrate, that the germs of different orders are not fortuitously dispersed in the interior of the animals, but each placed relative to the situation of the member whose loss it is to repair. Thus an arm, or a leg, never unfolds, where there should be a hand or a foot; nor do we ever see a hand originate, where a finger only is defective. This is proved, by simple inspection of the figures, and of 4, 5, 6, in particular.

I cannot think the reparatory germs are deposited in the osseous parts, or in those that may ossify; I rather suppose they are lodged in the soft parts, as more favourable to their evolution. While a germ unfolds all its parts, engraft, or anastomose with the corresponding parts of the original whole, and form one body with it; this union is evinced by the reparation of the mutilated member, since the new part exactly resembles it. Something analogous to vegetable grafts takes place, as M. Duhamel has well described.

The

(2) Vide *Considerations sur les Corps Organisés*, chap. 9. Haller, *Memoire sur le Poulet*,

The reproducing members of the newt and ſnail illuſtrate, that organic wholes may be completely developed without fecundation, in its proper ſenſe. The moſt ſubtile or active fluids of the animal are ſufficient to effect their evolution. Nothing need here be added to what is ſaid on the ſubject, *Contemplation de la Nature, Chap.* 3. *Part* 10.

Although the ſame member is mutilated five or ſix times ſucceſſively, it will be regenerated as often. Probably theſe ſucceſſive reproductions extend further; but we are yet ignorant of their limits. Theſe can be diſcovered by experiment alone; but it is evident that they cannot be infinite. In the ſame freſh water worm, I formerly ſaw twelve ſucceſſive reparations (1).

The

(1) *Traite d'Inſectologie.* 'It is very probable, that the 'property of theſe inſects, to renew a head or tail, when 'deprived of either, is proportioned to the number and 'nature of the accidents to which they are expoſed.'

In rivulets, I have ſometimes found worms that had loſt the head or tail, and then repaired it. Snails run the hazard of ſimilar accidents. Muller, at the end of his memoir on ſnails, *Journal de Phyſique* 1778, obſerves, that he found 'a naked ſnail in a wood, and two others, repairing a 'large horn that had accidentally been loſt.' I have had newts taken with deformities in the fingers and toes, which clearly indicated caſual mutilations.

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The members in miniature, which, though themſelves mutilated, produce a ſimilar, but ſmaller miniature, and the reproductions of this, when mutilated, afford a powerful ſupport of the hypotheſis of involution. However, I will not affirm that the reparatory germs are involved in each other; the expreſſion would be incorrect; but it appears that the expanding germ includes all the parts fit to reproduce a member, and, with theſe parts, germs united to them, which alſo grow, and that by their connection, and are deſtined to replace the loſt members. In ſhort, different generations of germs muſt not be conceived as boxes caſed in each other. The germ of the ſecond generation conſtitutes part of the germ of the firſt, juſt as a ſeed growing in a plant, or an egg in an animal, forms part of that plant or animal. The germ of the third generation conſtitutes part of the ſecond, and ſo on with the reſt. Thus all the generations included in the firſt germ are as many decreaſing parts of it, and it is itſelf a conſtituent part of the original member. I need not return to the objections againſt involution; they are anſwered elſewhere; and none ſimilar will occur to a philoſopher who knows the difference between hypotheſis and diſcovery.

MEMOIR III.

EXPERIMENT I. *Reproduced members mutilated.*—In the firſt experiment of the preceding Memoir, is an account of ſeveral ſucceſſive operations on a large newt, which were made purpoſely to diſcover whether reproduced members contained reparatory principles in the ſame way as the original members. There, it is obſerved, that the fourth mutilation was performed 13 October 1778. Another operation was performed on the ſame newt, 9 March 1779; the reproduced hand and foot being nearly as much advanced as thoſe the laſt time amputated.

We have ſeen that the third amputation was on the 24 of Auguſt, ſo that only fifty days elapſed between it and the fourth; whereas, between the fourth and fifth, there were one hundred and forty-ſeven, which is an additional proof how much reproduction is retarded by winter.

A new hand was announced by the appearance of new fingers, 21 April, and regeneration of the foot was at that time ſtill more ſenſible.

Three

Three toes could clearly be diſtinguiſhed, notwithſtanding their extreme minuteneſs. On the ſixth of May, only three fingers were viſible, but all well ſhaped; the longeſt equal to about two-thirds of a line. The three toes were nearly of the ſame length, fig. 1. 2. plate 11. The extremity of the arm and the foot are much ſwelled, fig. 1. 2. *r*.

It is here to be remarked, that neither fingers nor toes are in the ſame direction as the axis of the member, but, from being a little elevated, they are oblique, which is more conſpicuous in the latter.

June 1, the hand was exactly as in fig. 3. Four fingers were completely formed, the longeſt near a line and a half in length. The foot ſtill had only three toes, nearly as long as the fingers, fig. 4. I this day performed the ſixth operation, without waiting the reproduction of the other two toes, which ſeemed very tardy.

A new hand was viſible in the end of the month with three fingers, and a foot with four toes.

July 14, the hand ſtill had no more than three fingers, and the foot four toes; the longeſt toe about a line and a third; the largeſt finger a little leſs in length. It is almoſt unneceſſary to repeat, that all were whitiſh or greyiſh, and ſome tranſparent, as originating members uniformly

 are.

are. The toes had grown greatly against the 31, and darkened very much, but the increment of the fingers was less evident, nor was their colour so dark. Neither the fourth finger nor fifth toe yet appeared.

The seventh operation was now performed; and a remarkable monstrosity arose on the reproduced foot, fig. 6. 15 October.

The foot seemed to have renewed only three toes, and the second, *m*, was monstrous. It was evidently formed by the union of two; one-third of the length was thicker than the rest, and a cleft seen at the upper extremity, where the toes were not united.

The first and fourth finger were extremely short, fig. 5; the second and third more distant than they should have been, and the whole hand turned downwards, which increased the deformity.

I proceeded to the eighth mutilation, 29 October, no sensible change having taken place in the members since the 15, except in the colour becoming deeper.

While the amputated foot lay on my finger, I distinctly saw with my naked eye, and still better with a magnifier, a slight furrow or groove, all along the monstrous toe, and exactly in the middle, which seemed an indication of the place where the two toes engrafted or united, and produced

duced the monſtroſity. It is magnified, fig. 7; and the furrow, or groove uniting the toes, is plainly evident.

This is a moſt important fact, as it relates to the celebrated diſpute concerning the origin of monſters. Here we behold a graft by union, which nature has executed in a manner before the obſerver. The large horns of ſnails have preſented ſimilar inſtances, as appears from the horn, fig. 15, Plate 8. which aroſe from ſuch a graft, and it is proved by the two eyes at the extremity. But as the horns of a ſnail are originally ſofter than the members of a newt, and contain no oſſeous part, or what may oſſify, it naturally follows, that no indications of the junction will remain.

To my great regret, the newt died 27 November, probably from the bad quality of the renewed water. At that time, the animal had not exhibited any ſign of reproduction.

EXPERIMENT II. *A ſmall excreſcence cut from the leg of two newts.*—On the 28 of October 1778, I remarked that one of the large newts, which had only four toes on a foot, had, in place of the fifth, a very minute excreſcence, apparently an originating toe. However, I could not obſerve that it made any ſenſible progreſs; and I determined to cut it off, with the view of diſcovering whether nature would produce ſomething in its place, or regenerate the fifth toe. But nothing reſulted from the operation.

A ſimilar experiment was alſo made on another newt, and with as little ſucceſs. Undoubtedly the excreſcence was not the germ of a toe whoſe evolution ceaſed. It probably aroſe from ſome accidental cauſe: and as it did not include the germ of a toe, the exertions of nature were limited to ſimply healing the wound.

EXPERIMENT III. *On a monſtrous finger.*—On the 13 of July 1779, my newt catcher brought me one with the ſecond finger of the left hand monſtrous. There was a cleft at the extremity ſimilar to what had been obſerved in the newt of the firſt experiment, which alſo ſeemed to be formed by the accidental union of two fingers ingrafted four-fifths of their length by approximation.

Three days afterwards, the hand was drawn of its natural ſize, fig. 8. Neither it nor the fingers appeared to have been fortuitouſly mutilated: they were of the ſize, colour, and proportion peculiar to a hand fully developed, that had never been expoſed to any accident. Therefore the newt was probably produced with the monſtroſity. In the finger *m*, which is evidently larger than the reſt, is obſcurely ſeen the little groove, indicating the place of union. A wide angle is formed by the cleft, *b*, *b*.

While conſidering this ſpecies of monſtroſity, an experiment occurred to me for perfecting the theory of germs. I cut off one of the branches cloſe to its origin with very ſmall ſharp pointed ſciſſars, 3 July.

On the 19 of Auguſt, the finger had protruded a new branch half as long as the correſponding one, and evidently thicker: and on the 25, it was equal in length, but ſtill thicker.

This day, I cut off the unmutilated branch at its baſe. Reproduction advanced much ſlower; and not before the middle of October was it equal to the part replaced.

Both branches were amputated on the 21. My object was to diſcover whether the finger would reproduce a cleft ſimilar to the firſt: but an accident which befel the newt prevented the ſatisfaction of my curioſity.

However, it is proved by this experiment, that each branch contained a principle of reproduction, the evolution of which was determined by the operation.

I afterwards procured another newt with the ſame monſtroſity of the third finger. It was caught during ſpring of the preſent year 1780. On the 1 of September, I amputated the cleft at its origin: and a new one appeared on the 28. Thus each could be reproduced separately, and both at once.

Experiment IV. *Both hands and the left foot longitudinally divided.*—The experiments on longitudinal division, which afforded interesting results, are detailed in the preceding Memoir: It was of great importance to repeat one of this nature; for which several reasons have already been given. And I may here add another. By this mode of section, I thought a greater number of fingers and toes might be produced than nature allotted to each member. I conceived that longitudinal division might occasion the derivation of fluids favourable to the eruption of those germs that were around the edges of the wound. Thus it was principally with that view the following experiments were made.

July 13, 1779, the hands and left foot of a newt were longitudinally divided. From the 15, mould grew on the wounds, but I took care to brush it off with a pencil: and also to renew the water frequently. But, for all my precautions, half the hands and foot were consumed by gangrene. A minute trunk, formed by the metacarpus and metatarsus, was then visible at one side of the remaining part.

I

I never doubted that the trunk would produce new fingers and toes: my former experiments had prepared me for it: and in fact, I did see little papillæ on each trunk after an interval of some weeks, which announced the reproduction of parts in the place of those destroyed. The first two fingers of each hand were wanting, and the last three toes.

Two new fingers, about two-thirds of a line long, and in the natural position, appeared on each hand, 21 August. The second of the right hand exhibited a very singular monstrosity of the species already described. This finger was much larger than it should have been, and evidently arose from two germs grafted by approach. An originating cleft, similar to the others, was perceptible at the extremity.

The hand is magnified, fig. 9. *m* the monstrous finger: *d*, the other beginning to grow. The foot is of its natural size, but the originating toes, *n*, *n*, in an unnatural position, fig. 10.

Though the monstrous finger was examined with a considerable magnifier, nothing along the middle, which indicated the union of the two fingers, could be discovered: all was uniform. This, as well as the whole new fingers and toes, was semi-transparent, and of a whitish or greyish colour.

EXPE-

EXPERIMENT V. *The hands and feet of a newt longitudinally divided.*—On the 15 of July 1779, I divided the hands and feet of a large newt longitudinally. Mould soon appeared on the wounds: but by removing it with a pencil, and renewing the water, I saved the members.

The halves of the right hand and foot re-united: and the enormous wounds were so completely cicatrised, that it was impossible to observe the place of junction, at least in the hands: however, I thought I could perceive a long superficial furrow which might be it.

The left foot had lost the middle toe by the operation. But in a few weeks, there was discovered, in the middle of the foot, an extremely minute papilla among the four toes, which announced a new toe to supply the place of that destroyed.

I soon observed a similar papilla among the fingers of the left hand, which was recognised as a *medius*, unfolding exactly where the middle finger is in a hand with five fingers. But newts have only four; therefore, by a longitudinal section, this newt acquired five on one hand, and my object was happily accomplished.

The new toe was a full line in length; the new finger only half a line. Both the hand and foot were then designed, fig. 11, 12. In consequence of dividing the hand, the second finger is farther from

from the third than ufual, fig. 11. Between them is a middle finger ftill very fmall, but evidently a new one, *m*. From the fame caufe, the fecond toe is alfo farther from the third than it fhould be. Between them is feen the new toe, *n*, which is to replace that which was loft.

The wound on the metatarfus was now fo well healed, that no marks of it remained. Thefe immenfe wounds being healed by a kind of ingrafting, which effects the union of the parts, is no inconfiderable corroboration of a theory which I have fuggefted on the formation of certain monfters, that feem to originate from the accidental union of two organic wholes. Nor is the fupernumerary finger, which I was able to give the newt, lefs favourable to it.

EXPERIMENT VI.—We muft not conclude, from the experiments in this and the preceding memoir, that, in confequence of longitudinal divifion, a new finger will always arife in the middle of the member, and one, in this manner, have it in his power to produce a fifth finger on hands, and a fixth toe on feet, at pleafure. Experience has taught me, that cleaving the members afunder is not uniformly followed by an additional reproduction. Many circumftances, which we are yet unable to afcertain, may deeply influ-

ence

ence the effects of the operation; and of these, the following is an example.

By a cut as far as the tarsus and carpus, I divided the hands and feet of a large newt longitudinally, 25 August 1779. After the operation, the parts separated far asunder. I expected much mould on the wounds, but so little appeared that it was unnecessary to remove it. The middle toe of the left foot was destroyed.

In about six weeks, no traces of the wounds remained. The third and fourth finger of the left hand, and the third and fourth toe of the left foot, were more distant from each other than naturally. The middle toe of this foot was replaced by another, which was about two lines long, 29 October.

Experiment VII. *Various monstrosities arising from the amputation of members.*—Here I shall subjoin some remarkable instances of monstrosity arising from the amputation of members.

A foot which had produced six toes, but three of them united great part of their length, is represented, fig. 13. The same foot is magnified, fig. 14, to shew the monstrosity better. On comparing this with fig. 8, 9, the monstrous toe evidently exceeds the natural size, was it not formed as the others by the union of two. Here there are two clefts instead of one. The branch

a,

a, is longer than *b*, *c*: the middle one *b* is shortest. No indication of the union was perceptible, it was ſo complete.

Another newt, whoſe hands and feet were tranſverſely amputated, 8 June 1780, preſented a monſtroſity ſtill more ſingular. The four members began to reproduce in the end of July. Conſidering the newt attentively, towards the firſt days of Auguſt, I was agreeably ſurpriſed to ſee eight toes regenerating on the left foot. It was neceſſary to hold the foot in a particular poſition and examine it cloſely, as they could not be diſcerned at the firſt view.

This monſtrous foot was deſigned Auguſt 9. The metatarſus, *m*, is a little larger than common; and the fifth toe, *c*, ſeems to riſe above the fourth, *p*, fig. 15.

There was alſo a ſingularity in the right hand. It reproduced five fingers; the firſt two much ſhorter than the reſt. It was deſigned 10 Auguſt, fig. 16.

The regenerated right foot was likewiſe deformed; the laſt two toes being connected by a membrane, like that of ſome aquatic animals. Thus the only perfect part of the newt was its left hand.

Again obſerving the monſtrous foot, fig. 15, with the greateſt attention, November 7, I diſcovered that the fourth toe, fig. 17, 5, 6, 7, was compoſed

composed of three longitudinally united, and nearly as in fig. 14. The third toe, also, was clearly formed by the union of two, but joined only a small part of the length. All this gave the reproduced foot a very uncommon appearance, and occasioned a confusion, which prevented the figure and arrangement of the parts from being easily understood.

Is it not indicated, by these frequent graftings, that the reparatory germs of mutilated parts are situated very near each other, in the interior of the members, because they can only arise in consequence of such proximity? Such remarkable instances enable us to judge how many operations may be performed on newts, well calculated to elucidate the mysterious origin of monsters. It is proved that reproducing members, if mutilated, produce similar members. Thus may various monstrosities be regenerated. If the eight toes of the monstrous foot were amputated, which is an experiment that I mean to make, eight toes resembling the first would most probably be reproduced; and reproduction might perhaps extend further, by encroaching a little on the metatarsus.

Experiment VIII. *The limbs of a newt dislocated.*—Sig. Spallanzani having requested me to dislocate

dislocate the limbs, I did not fail to make this experiment. But it is much more difficult to dislocate a limb than to amputate it; for the pliancy and lubricity of the members contributes to render the experimentalist's exertions abortive.

On the 13 of July, I performed this new operation on two large newts, at half past two. It is of some consequence to attend to the exact hour. Both arms of the one and both thighs of the other were dislocated. The operation was indubitably complete; for, besides the sensation felt in performing it, which convinced me of the fact, the members immediately afterwards were pendent, as if dead, and the animal had no longer any power over them, which was an unequivocal proof.

At six in the morning, on the 14, it was impossible to recognise any symptom of dislocation. In the evening, each newt moved the disjointed limbs with a liberty and facility which announced that nature had already repaired the disorder.

EXPERIMENT IX. *On the eyes of newts.*— This and the preceding experiment are cruel indeed; and sensible minds will hardly pardon the observer's cruelty, though it arises from an evident

dent desire to discover new facts and enlarge our knowledge of the animal œconomy. Therefore, I fear the compassionate reader will revolt further still at what is yet to be related. But I beg he will consider, that animals, which, after losing one, or even several limbs, continue greedily devouring the prey presented, undoubtedly cannot experience the sensation of pain to the excess which our own sensations lead us to imagine. We are very insufficient judges of what passes within an animal so remote from us in the scale of living beings. Let it not be thought, that by these reflections, I mean to lessen the natural repugnance of every humane mind to make animals suffer. The benignity of nature itself will inspire man with this precious sentiment to prevent the enormous abuse that his power might exert over the animals which she has subjected to his dominion. Yet let me ask, whether a rational person abuses his empire over animals, by making them suffer only for his own instruction, or that of his fellow creatures.

With a scalpel, I extracted the right eye of a large newt, 13 September 1779: but I did not obtain the globe without much injury to the tunics. It was the first time of performing the operation, and before I had acquired the peculiar dexterity necessary for success, and afterwards learned

learned by experience. Thus the utmost disorder ensued in the eye, and the chrystalline lens started out on my nail. This is a beautiful object; no larger than a millet seed, and quite transparent. I thought that I beheld one of the spherical lenses with which Leeuwenhoeck discovered so many wonders. But contact of the air soon tarnished the minute lens; it dried and became disfigured.

A deep bloody wound in the socket of the eye was the consequence of this cruel operation. And the reader will not be surprised if I hardly expected any thing from it, and that the newt would probably remain blind for ever. How great was my astonishment, therefore, when, on the thirty-first of May 1780, I saw a new eye formed by nature. The iris and cornea were already well shaped, but the latter wanted its peculiar transparency, which is very considerable in these animals. Impatience to arrive at the most important part of the prodigy has induced me to omit the progress of it; and observe that nature certainly began with closing the wound.

The eye was completely repaired 1 September. The cornea was nearly as transparent as that of the other eye, with which it was frequently compared. The iris had also acquired the yellow gilded colour, which characterises this species of newt. In short, the eye was so perfectly renewed, that no veftige exifted of the uncommon operation that the animal had undergone. During the remainder of this and the following month, the cornea always became more tranfparent; and now, when I write thefe remarks, 8 November 1780, it is equally perfect as the other: but the reproduced eye feems a little fmaller than the entire one; and the iris, or golden circle, goes only half round the ball (1).

It would ftill be neceffary to extract the reproduced eye, to afcertain, by diffection, whether it contains a chryftalline lens fimilar to the original. But I confefs, that, as yet, I have not had refolution to fubject the newt to the moft barbarous of all operations; and I fhall probably await its death for fatisfying my curiofity.

A newt is mentioned in the fecond memoir, which had loft the regenerated members four times, and had taken no food for feveral months. It became fo much emaciated, that the joints of the fingers could be feen through the flefh. The head

(1) According to Blumenbach's obfervations, this experiment has failed when the whole globe of the eye was extracted. In fix months, after difcharging the humours, and cutting out the tunics, the eye was reproduced. Even after eleven months had elapfed, it was not fo large as the other, *Specimen Phyfiologiæ Comparatæ*.—T.

e 434.

head had ſhrunk in ſuch a manner, that the eyes projected like two lumps, and appeared much larger than uſual. It died 11 November; and I extracted an eye. The opaque cornea or ſclerotides, *o*, fig. 18, plate 11, is of an iron grey; *p*, the ball; *c*, the tranſparent cornea; *n*, the optic nerve. The eye is ſhaped like a flattened ſpheroid, the edges of the ſclerotides forming the equator. But my purpoſe is not to deſcribe this organ.

ADDITIONAL NOTES AND OBSERVATIONS

BY THE

TRANSLATOR.

INTRODUCTION, p. 13.—Trufting to Linnæus, it is faid the tortoife lays 1000 eggs. I fhould rather fuppofe that learned naturalift is correct, but I have found no authority which makes the animal nearly fo prolific. However, there is a great difference in the numbers which the females of very fertile animals will produce. I have in fome obferved intermediate numbers from fifty to a thoufand eggs.

TRACTS, vol. 1. p. 50.—The author's neglect, or rather contempt of nomenclature, occafions confiderable difficulties to thofe who ftudy his

 works.

works. Indeed, all the ſyſtems we have are ſo brief and indiſtinct, that even the moſt expert naturaliſt will ſometimes be at a loſs to diſcover whether the animal there named is truly the object of his ſearch.

It is the *Oeſtri* that depoſit their eggs in the ſkin of cattle; and great pain is occaſioned to the animal by the ſwelling of tumours, which include the larva of the inſect. *Aſili* are a different genus of flies.

P. 84.—Since the note on the torpidity of fiſhes, I obſerve that the fact is confirmed by late obſervations, which will be found in CEPEDE *Hiſtoire Naturelle des Poiſſons*, Tom. 3. Paris 1802. ‘ Formerly we had vague ideas concerning mackrel in their ſub-marine aſylums during ‘ winter, and particularly in the polar regions; ‘ but we are now indebted to vice Admiral Pleville le Peley, for certain information. He has ‘ verified his obſervations along the coaſt of ‘ Greenland, Hudſon's Bay, and towards the ‘ banks of Newfoundland. In theſe northern ‘ countries are arms of the ſea, named *baracbouas*, which are ſo interſected by points croſſing ‘ them, that at all times the water is as calm as ‘ in a baſin. The depth of theſe places is va-

‘ rious,

'rious, according to the proximity of the banks, 'and the bottom is generally covered with soft 'clay and marine plants. It is here that the 'mackrel lie in concealment during winter; and, 'thrusting the head and anterior part of the bo- 'dy about a *decimetre* into the mud, their tails 'are kept perpendicularly above it. Thousands 'are found thus buried in each *barachoua*, and 'cover the bottom of the basin in such a man- 'ner, that seamen, unacquainted with the coast, 'have been afraid to approach what they thought 'were shoals. In Citizen Pleville's opinion, the 'surface of these *barachouas* is frozen in winter; 'and the thickness of the ice, as well as the snow 'above it, moderate the effects of the season, and 'contribute to preserve the animals alive. Only 'towards July do the mackrel recover part of 'their activity, depart from their holes, and, com- 'mitting themselves to the waves, traverse the 'great banks. It even appears that this stupor, 'or torpidity, is dissipated by degrees. Their 'senses are feeble for twenty days; their sight is 'so weak, that they seem blind; and they are 'easily caught in nets; afterwards they can only 'be taken with the hook, but being emaciated 'from long abstinence, and greedy for the bait, 'great numbers are caught,' page 32, 33.

 P. 207.

P. 207.—Though M. Bonnet is of opinion that the incorporation of foreign particles removes the original tranſparence of animals, it is probably owing as much to chemical combinations.

Seminal Vermiculi.—It is yet uncertain by whom theſe animals were firſt diſcovered. Different authors aſcribe the diſcovery to various naturaliſts who lived towards the end of the ſeventeenth and in the beginning of the eighteenth century. However, it is undoubted, that Leeuwenhoeck was the firſt who inveſtigated their properties.

It was generally believed, that all ſeminal vermiculi were of the ſame ſize; that thoſe of the frog were as large as the vermiculi of the horſe. The author has now put it beyond diſpute, that there is not only a difference in the ſize of thoſe of different animals, but that all the vermiculi of the ſame animal are not equally large.

Vol. 2. *Wheel Animal.*—This animal I have often found in ſituations without the ſmalleſt particle of ſand: indeed, it has repeatedly appeared in different infuſions, and in great numbers. But I do do not recollect to have met with it in any liquid with a tendency to putrefaction.

The horn of the wheel animal is situated on the upper part, and is not easily observed unless when the animal is just about to make its step.

P. 163, Note.—Before terminating this work, it is proper to correct what is probably an error. It is possible, that what I conceive a new species of *sloth* is not so. Some points in its history have already been attended to by naturalists of great eminence.

In the French translation of *Fuessli's Archives of the History of Insects*, Winterthour 1794, there is a memoir on these minute animals by the celebrated Muller. His observations coincide almost exactly with mine; and he seems to have had the advantage of a number for examination.

He observes, in the first place, that 'Eichorn 'and Goetze at the same time discovered the ani-'mal, but the former ascribed ten feet to it instead 'of eight; and the name they bestowed upon it 'was the URSLET (*Ourslet ou Petit ours*), from 'the supposed resemblance to a bear.'

In

In Muller's opinion, it is not an infusion animalcule, 'though its proper abode is among 'them, on the water lentil. The figure and 'number of feet approximate it to the genus of 'mites or acari; and although neither eyes nor 'antennæ are perceptible, the other parts induce 'us to clafs it with infects.'

He proceeds: 'Three claws terminated each 'foot; something like eggs were feen within. 'Sometimes I found a fimple fkin with the feet, 'nails, and eggs. This is not a dead *urflet*, but 'a flough, fuch as other animals throw off; but 'how does the ovary come here?'

The figure in the Archives, Plate 36, feems very correct. If we may judge from what is faid in the account of it, this animalcule muft have been a great deal larger than mine.

Whether it belongs to the clafs of infects may be doubted: if it does, I fee few reafons why Spallanzani's does not belong to it alfo.---It is very probable, that feveral of what we call *infufion animalcula*, are only miniatures of fome larger animals.---Thofe of the northern nations have alone been inveftigated; it is reafonable to expect new animalcula as well as we find new ani-

mals

mals in other countries, or new connections with those already known.

Boat Worm.—I am uncertain what animal, according to the Linnæan nomenclature, the author calls the boat worm. The third chapter of his *Prodromo* is appropriated to its reproductions; and he thus describes the animal itself. 'It is 'annulated like the earth worm, by which means 'it can contract, extend, and move from one 'place to another. Towards the head, it is 'as thick as a large goose quill. It is generally 'about eight inches long, but the largest, when 'extended, are a foot. The colour of the back 'is a mixture of grey and brown, which becomes 'lighter towards the tail: the belly is whitish.

'The ordinary abode of this animal is in clear, 'shallow, or gentle running water. The anterior 'part is buried in the mud, but the posterior rises 'to the surface of the water. There it enlarges 'and curves below like a boat which extends on 'the surface. The concave part of the boat is 'uppermost; and the rising of the edges above 'the water seldom allows it to flow over. With-'out this particular figure it would sink, as it is 'specifically heavier than water.—These peculia-'rities have induced me to call it the *boat worm.*'

The

The fresh water worms, so often mentioned, seem to be the *Lumbricus Variegatus*, and *Tubifex* of Linnæus. A full account of them will be found in the first volume, *Œuvres de Bonnet*. The sea nettles are *Medusæ*. But the reader will observe, that, throughout this work, the original names are preserved, without using those in the *Systema Naturæ*, from the hazard of error, and the same with respect to the different elastic fluids.

ANALYTICAL

ANALYTICAL INDEX.

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LETTERS

LETTERS FROM M. BONNET.

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Death

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ANIMAL REPRODUCTIONS.

Reproduction

MEMOIRS BY M. BONNET.

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